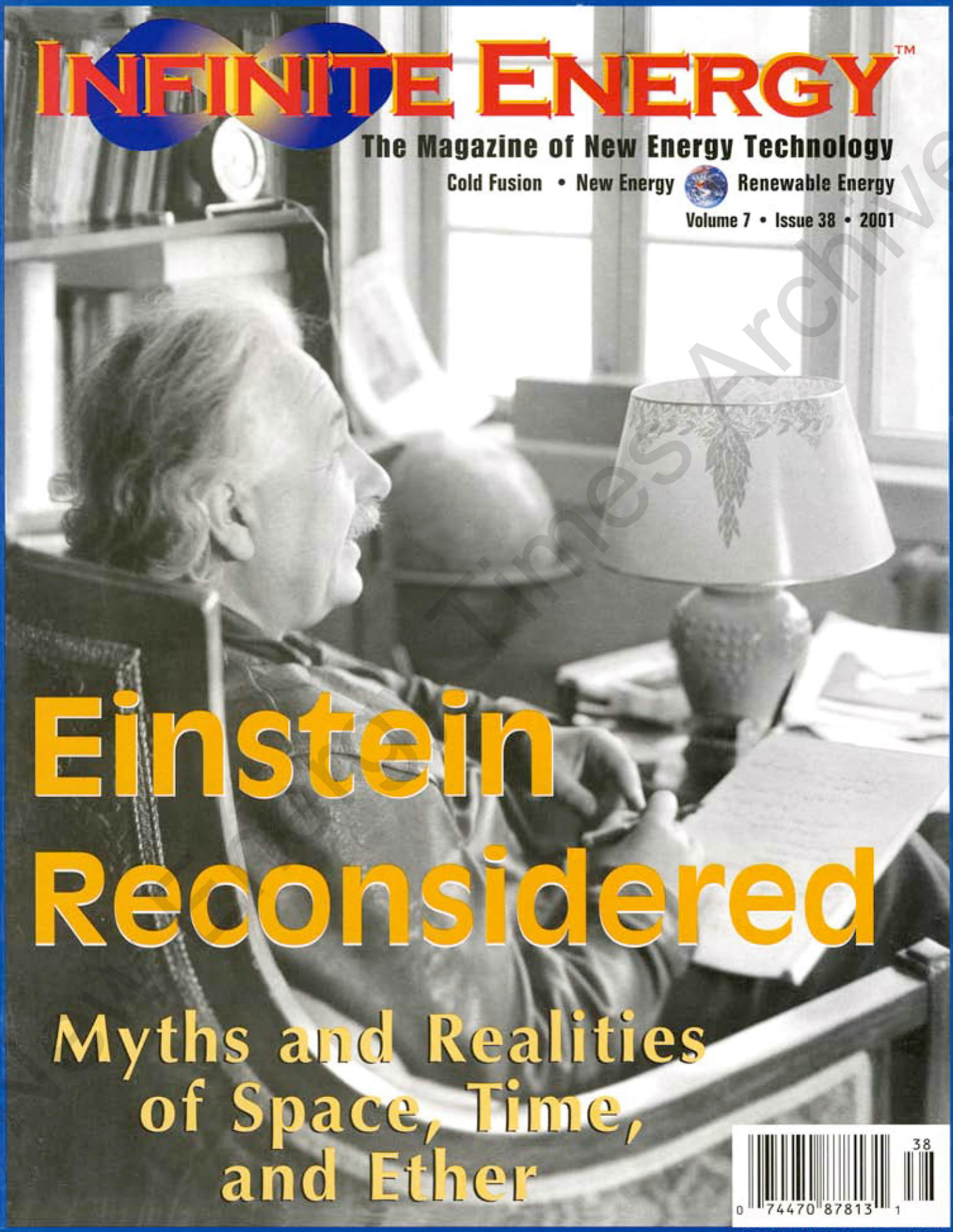

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The Magazine of New Energy Technology

Cold Fusion • New Energy • Renewable Energy

Volume 7 • Issue 38 • 2001

Einstein Reconsidered

Myths and Realities of Space, Time, and Ether

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THE EXPERIMENTAL AND THEORETICAL WORK OF THE CORREAS

AT THE *AURORA BIOPHYSICS RESEARCH INSTITUTE (ABRI) & LABOFEX — EXPERIMENTAL AND APPLIED PLASMA PHYSICS*

"At Labofex we pursued an experimental investigation of the electrodynamics of anomalous cathode reaction forces made manifest when the abnormal glow discharge was conditioned to pulsate autogenously by auto-electronic emission. Aspden's law fully accounted for the phenomenon, since it was the first and only law that treated the interaction as being subject to the ratio of the masses of charge carriers. But in our aetherometric studies at ABRI we encountered electrodynamic interactions involving ambipolar charges devoid of inertia. This obliged us to rethink electrodynamic interactions from scratch, as it were."

"Aetherometry, or the new metric science of massfree energy, proposes a comprehensive approach to physics and biophysics, based upon an energetic treatment of the manifolds of Space and Time, and directly accounts for the fine wave and particulate structure of all energy manifestations, including mass-energy. We designate this unitary approach and its functionalist physico-mathematical method, as the Aetherometric Theory of Synchronicity (AToS), emphasizing its analysis of Time as a manifold that is distinct in metric and quality from that of Space. Experimental Aetherometry unravels the discovery process that led us from a re-examination of the thermal and electroscopic anomalies of the ORAC (orgone accumulator), through a reevaluation of electromagnetism, to the rediscovery and reproduction of Reich's OR motor and our invention of an improved Aether Motor. Theoretical Aetherometry provides the critical tools we required to understand and correlate the electrodynamic, thermodynamic and gravitokinetic phenomena we experimentally encountered. It became a unitary and integral methodology when we realized that it permitted a totally new way of analyzing and measuring physical interactions, from which we have been able to extract such basic physical knowledge as — the exact spectrum of hydrogen, the fine wave and volumetric structure of the electron and proton, a new determination of the fine structure constant, the equivalence of charge with linear momentum, the reduction of all physical units to the dimensions of length and time, an exact quantum expression for G, the identification of a periodic table of gravitons, a new law of electrodynamics, and a pure wave treatment of the different radiative forms of aether energy. This permitted identification of the exact solar spectrum of aether radiation and its velocity distribution — which is in no way limited by c — as well as the derivation of all blackbody spectra, including the CBR. We regard AToS as the discovery of the precise code for the actual workings of all energy manifestations, whether massbound or massfree, and this may well prove to be an even more momentous leap for our understanding of nature than was the discovery of the genetic code for biological systems."

Paulo Correa, MSc., Ph.D.

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INFINITE ENERGY

The Magazine of New Energy Science and Technology

A special issue awaits you: "Einstein Reconsidered." It begins with "The Einstein Myths" on p. 6

Photo: Courtesy Northeast Sustainable Energy Assoc. Home, Jack Groh, Rhode Island.

The number of people venturing off of the commercial electricity grid is growing. . . p. 44

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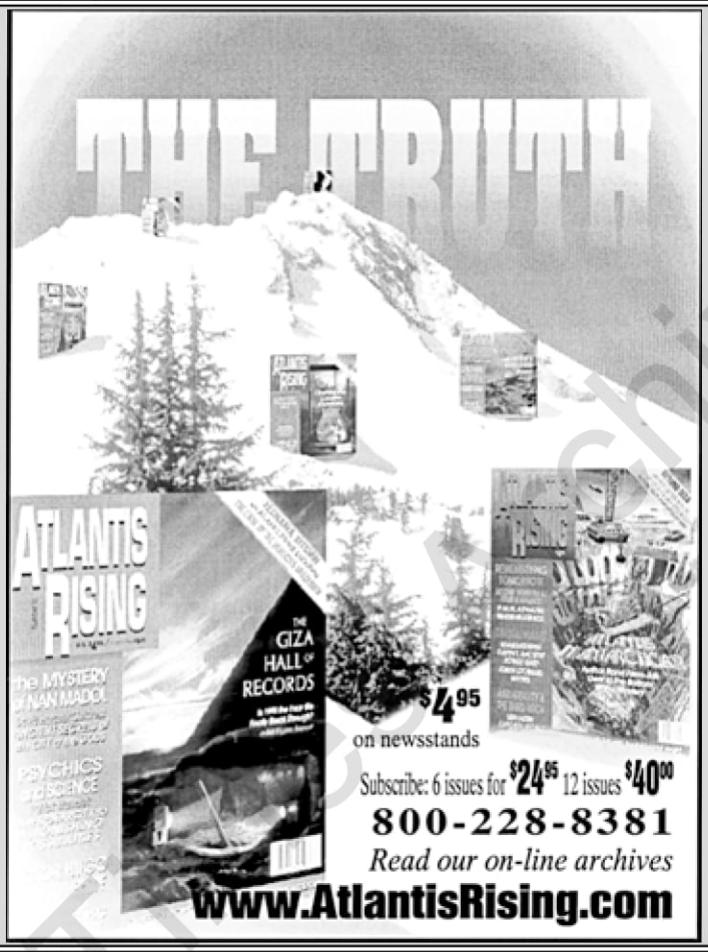

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LETTERS TO THE EDITOR

Time Magazine Off Track on Einstein

I am sending you a copy of a letter that I had mailed to Time magazine in response to their selection of Albert Einstein as "Person of the Century." As you probably know, Time published nine letters in the January 24, 2000 issue, none critical of their choice. —Pari Spolter

In the introductory article "who mattered and why," *Time* magazine Managing Editor Walter Isaacson mentions this century's pioneers in science and technology. Missing from the list were: British physicist Louis Essen (1908-1997), the inventor of the atomic clock at the National Physical Laboratory at Teddington, Middlesex in 1955; Bill Gates, the developer of the Disk Operating System, and NASA's unprecedented achievements in space exploration and maintenance of the system of satellites that makes global phone and Internet communications possible.

Yes, the kindly professor with a wild halo of hair, piercing eyes, and engaging personality, who traveled with his violin and played Mozart, was a sympathetic figure. But giving Einstein credit for the remarkable advances in science and technology, most of which occurred during the latter half of the century, is erroneous.

Referring to the paper on the special theory of relativity, you admit: "It was based, like much of Einstein's work, on a thought experiment. . ." The revolutionary advances in technology, which have transformed the world in the past half-century, are the result of real experiments.

The unveiling of the mysteries of the universe is an ongoing process. Einstein proposed his theories. The special and general theories of relativity have been controversial. I have discussed some serious flaws in these theories in Chapters 2 and 3 of my book, *Gravitational Force of the Sun*. There is an extensive survey of the literature, with references to numerous articles and books by many authors who have criticized the relativity theories. The Michelson and Morley (MM) experiment did not provide direct evidence for length contraction or for time dilation. The logical conclusion from the MM-type experiments is that an assumption of the simple additive effect of velocities, $c \pm \upsilon$, is incorrect when dealing with electromagnetic radiation. Michelson and Morley measured the distance. They did not measure a change in the wavelength. In recent experiments, Silvertooth and Whitney[1] have used a standing wave sensor to scan through the laser beam and detect the position of the nodes with high precision. They found that the spacing between the nodes varied with orientation of the apparatus and with the time of day. For calculation of the velocity of the solar system, they have used the Doppler formula rather than simple addition of velocities used in the Michelson-Morley paper and they have detected the motion of the Earth.

Missing from the twenty-four page article was any mention of critical comments such as: "The general public is misled into believing that science is a mysterious subject which can be understood by only a few exceptionally gifted mathematicians"; "[The] continued acceptance and teaching of relativity hinder the development of a rational extension of electromagnetic theory"; "Students are told that the theory must be accepted although they cannot expect to understand it. They are encouraged right at the beginning of their careers to forsake science in favor of dogma," and "There have always been critics: Rutherford treated it as a joke. . . I do not think Rutherford would have regarded the theory as a joke, had he realized how it would retard the rational development of science"[2,3] by the distinguished British physicist Louis Essen, the inventor of the atomic clock and a fellow of the Royal Society who was forced into early retirement because he dared to publicly express his opposition to the relativity theory.

Verification of Einstein's theories, or dreams, is costing the taxpayers a great deal of money. In the 1960s, Joseph Weber, a professor of physics at the University of Maryland, claimed he had detected gravitational waves, which were predicted by Einstein's general theory of relativity. During the next decade, fifteen other research groups around the world obtained negative results even with markedly improved sensitivities of the instruments. These negative results did not discourage the diehard supporters of Einstein's general theory of relativity. They just said that they needed more money for outrageously gigantic equipments to reach higher precision. At a cost of more than $300 million, two separate installations of Laser Interferometer Gravitational Wave Observatory (LIGO) are under construction now. Each installation consists of two pipes 4 km long. Another boondoggle project, funded by taxpayers for more than thirty years, is the Stanford Gyroscopes, or Gravity Probe-B. They propose to measure a precession of 0.042 arcseconds per year at an altitude of about 650 km, to test Einstein's general relativity. When you consider that solar wind at that height sometimes reaches speeds as high as 800 km s^{-1}, the futility of the enterprise is obvious. These projects are diverting the needed funds from other more important research, such as harnessing of solar energy and other means of producing clean and affordable energy and research on climate control to find out how to prevent tornadoes and floods and how to make rain when and where it is most needed for agriculture.

When some scientists send a paper questioning the validity of the theory itself, they cannot get their paper accepted in any scientific journal.

The selection was rigged. The obvious choice for "Persons of the Century": The winners of the 1956 Nobel Prize in Physics, John Bardeen (1908-1991), Walter Houser Brattain (1902-1987), and William Shockley (1910-1989), the inventors of the transistor at the Bell Telephone Laboratories in New Jersey. This invention, more than any other, launched the revolutionary advances in technology in the past half-century.

Pari Spolter
Granada Hills, California

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Express Their Doubts," *Wireless World*, 84, 44-45.
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Oceans of Free Energy

In Eugene Mallove's editorial in *IE* No. 36 about oceans of free energy, he seems to have missed the point of our paper, "Solar Energy Liberation from Water by Electric Arcs," in *IE* No. 33. This involves the thermal energy of the atmosphere without violating any laws of thermodynamics. Figure 6 in the paper indicates that solar energy (environmental heat) evaporates cold fog to atmospheric water vapor. The vapor later condenses to water droplets in the clouds and in so doing it absorbs heat from the atmosphere. The absorbed energy is almost immediately replenished by solar heat supplied to the atmosphere.

It has never been denied (see book review of *A Biography of Water* by Philip Ball in *IE* No. 33) that the hydrogen bonds of liquid water store potential bond energy. What textbooks on chemistry fail to do is to state what supplied the stored bond energy in the first place. The source has to be kinetic energy given up during condensation. This kinetic energy the molecules acquired in heat exchange with air. It disappeared in the so-called latent heat of condensation. The textbooks then mislead students to believe that bond energy is not something stored in the bond, but the thermal energy required to break bonds. Confusing stored bond energy with thermal bond dissociation energy has concealed what you called the ocean of environmental energy which we have tapped experimentally and hope to tap commercially.

The same issue arises in the MHD power generation of air arcs which received the first public mention in Issue 25 of your magazine under the heading, "Why Does Lightning Explode and Generate MHD Power?" In the last three years we have made good progress in our two laboratories with air arc MHD generators.

The nitrogen molecule (N_2) contains an extremely strong bond between two nitrogen atoms which makes the N_2-gas behave almost like an inert gas. This strong bond is associated with a large store of potential bond energy due to the repulsion of the two nitrogen nuclei. Pushing two nuclei together against their Coulomb repulsion is like storing potential energy in a compression spring. The compression is the result of electronic forces of attraction, suspected to be spin coupling in shared orbitals. The electronic attraction can easily be switched off by electrodynamic forces in the arc. This then allows the nuclear repulsions to explode the air.

Textbooks do not dispute that the strong electronic coupling in N_2 must produce equal and opposite nuclear repulsion to prevent nuclear fusion. It then follows from simple Newtonian mechanics that the compressed nuclei have to store a commensurate amount of potential energy. Once more the textbooks do not specify the source of the stored bond energy. Since the relative motion of the two nitrogen atoms is arrested at the instant of bonding, there can hardly be any doubt that the kinetic energy of the two atoms is converted to stored bond energy. Environmental heat is, therefore, used up in the process of bonding. This is later replenished by the Sun and liberated in the MHD generator.

Your problem is that chemists will not say what the origin of stored inter atomic bond energy is. Thereby they throw a cloak over your environmental energy ocean, making you believe that a perpetual motion machine is required.

Peter Graneau
Concord, Massachusetts

Editor's Reply:

The arc discharge work of Peter and Neal Graneau, with both water and air, has been very pioneering. We are happy to have showcased it for our audience earlier in *IE* No. 13/14 and No. 33. I appreciate Peter Graneau's comments about where he believes the excess energy of his arc experiments may be coming from—stored chemical bond energy. But he should not confuse the "ocean of environmental energy," of which I subsequently spoke editorially in *IE* No. 37, with his conceptions of environmentally available energy (bond energy, deriving ultimately, he says, from solar power).

The environmental energy of which I spoke is made available through previously unnoticed (or repressed!) physical processes that are in defiance of the Second Law of Thermodynamics. As one example, the pioneering work by Dr. Paulo and Alexandra Correa, building on the work of Wilhelm Reich, seems to evidence this kind of energy (*IE* No. 37, pp. 12-22; and now see www.aetherometry.com). Then there is also the research of Dr. Xu Yelin in China and Maurizio Vignati in Italy, also reported in issue No. 37, which are more direct affronts to the Second Law. These have nothing to do with an "energetic aether." But the Correas will also be discussing their conception of "latent heat" in the atmosphere, which they suggest originates from an entirely different context in new physics. Perhaps their website discussions about latent heat will provide a different theoretical perspective to the excess energy that Peter Graneau believes his group is observing.—*EFM*

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A Fourth Ocean of Energy

I just read the "Breaking Through" editorial in *IE* No. 36 about oceans of energy. I believe that there is a fourth ocean. I believe that it may have been stumbled into in the past, and never understood. I suspect that one can make a PPM-4 which draws energy from the magnetic field of the Earth. The point of concern, though, if an ocean of such a nature could be tapped, would be the effect on the speed of rotation of the Earth. Albeit this is mostly presumption, but were this to exist, and if it affected the Earth as I venture to guess, then it would have to be universally banned from use. Seems absurd to simply ban a form of energy, but the consequences are too large.

Whew, I just landed with my feet back on the ground.

Chris Eddy
Pittsburgh, Pennsylvania

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Nuclear Research is Stalled

Historically, the Department of Energy (DOE) is hostile to science originating outside the control of the DOE's ten National Laboratories. An elitist example is a Fermilab poli-

Letters continued on page 94

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The Einstein Myths: Of Space, Time, and Aether

Einstein, the most celebrated scientist of the twentieth century, remains an icon of the power of human reason to penetrate mysterious nature. For billions of people who have been taught the essence of his relativity theories, he changed (or muddled) their very conceptions of time and space. He destroyed the common sense concept of a universal now—the absolute simultaneity of events in different relatively moving reference frames. Physicists grant Einstein full credit for having abolished, at least while his influence has reigned, the pervading "luminiferous aether," which was the medium for the transmission of light waves—universally accepted in the nineteenth century.

Will Einstein's stature extend far into the twenty-first century? Not likely. Enduring will be his justified fame for: 1) explaining the Brownian movement (the visible jostling of particles in liquid suspension from molecular buffeting), which effectively ended the debate about the existence of atoms; 2) his quantum explanation of the photoelectric effect (for which he won the 1921 Nobel Prize in Physics); and 3) his well-known social conscience and beliefs in what has been called "cosmic religion." He is also justly famous for his extreme displeasure with the probabilistic underpinnings of quantum mechanics. ("The theory yields much, but it hardly brings us closer to the Old One's secrets. I, in any case, am convinced that He does not play dice."—from a 1926 Einstein letter to Max Born) But believers in Einstein's infallibility will be lucky if the physicist's relativity theories survive beyond 2005, the 100th anniversary of his so-called *annus mirabilis* (1905), the year in which his Special Theory of Relativity and two other major works were published in Germany's *Annalen der Physik*.

For all their apparent predictive power, Einstein's relativity theories are deeply flawed, as the critical papers in this first of two *Infinite Energy* special "Einstein Reconsidered" issues will demonstrate formally. Einstein criticism is, of course, not new. (We are obviously not referring to Nazi-inspired, anti-Semitic tracts against relativity that were published in the 1920s, which disparaged his relativity theory as "Jewish science" or worse.) There are many sources of technical critiques of Einstein's work, such as the dissident journals *Galilean Electrodynamics*,[1] *Physics Essays*,[2] *Apeiron*,[3] *Journal of New Energy*,[4] etc., as well as books by thoughtful critics: Harold Aspden,[5] Petr Beckmann,[6] Peter and Neal Graneau,[7] Ronald Hatch,[8] Herbert Ives,[9] Thomas Phipps, Jr.,[10] and Franco Selleri,[11] to name but a few. There is even an organization, the Natural Philosophy Alliance (NPA),[12] which holds regional and national meetings devoted to critiquing modern physics, especially Einsteinian relativity. This community of dissidents and publications has been completely ignored by a self-satisfied Physics Establishment, which preserves its power and prestige, in part by mystifying veritable "scientific saints," such as Einstein and Stephen Hawking.

The Einstein Memorial at the National Academies, Washington, D.C.

What is very new in Einstein criticism, however, is a body of emerging experimental evidence for an energetic aether, which could be tapped to run electrical machines and generate anomalous heat. Actually, it is the *re-emergence* of this evidence for an energetic aether after it was rejected by officialdom in the 1940s and 1950s. Also, a handful of theorists have come to believe that aether-based models of subatomic structures are necessary to explain the anomalies in the cold fusion/low-energy nuclear reaction field. The last issue of *Infinite Energy* featured the landmark article by Dr. Paulo and Alexandra Correa, "The Reproducible Thermal Anomaly of the Reich-Einstein Experiment Under Limit Conditions" (p. 12). This told of Albert Einstein's inappropriate explaining-away of an important thermal anomaly associated with Faraday cages (metal boxes) after the phenomenon was brought to his attention in early 1941 by Wilhelm Reich. If this and related electrical anomalies evidencing *mass free charge* from an energetic aether are real, as I for one am reasonably sure they are, then it is clear that the standard conceptions of physics, particularly Einstein's relativity theories, cannot be correct. This, despite their elegant foundation in only a few postulates, such as the *relativity principle* relating specifically to electromagnetism (which Einstein borrowed from Henri Poincaré) and the supposed constancy of the speed of light in vacuum with respect to any observer, which was his own invention.

Einstein's Doubts

Einstein himself at various times had expressed doubts about the edifice of modern physics that he had helped to create—witness the remarks that follow. Perhaps his most serious expression of doubt came in a 1954 letter, the year before he died, to his friend Michel Besso: "I consider it quite possible that physics cannot be based on the field concept, *i.e.* on continuous structures. In that case, nothing remains of my entire castle in the air, gravitation theory included, and of the rest of modern physics."[13] Biographer Abraham Pais hastens to excuse this slip from contemporary certainty about relativity theory, claiming that virtually all physicists

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think that this self-assessment at the end of Einstein's life was "unreasonably harsh." But just a few years earlier (1948), in an introduction to a popularized book about relativity, Einstein was also circumspect about physics, in a more general sense: ". . .the growth of our factual knowledge, together with the striving for a unified theoretical conception comprising all empirical data, has led to the present situation which is characterized—notwithstanding all successes—by an uncertainty concerning the choice of basic theoretical concepts."[14]

In my estimation, Einstein was a person much more cautious about dogmatic expression than those who have claimed invincibility for his relativity theories. In a letter to J. Lee in 1945, Einstein wrote: "A scientific person will never understand why he should believe opinions only because they are written in a certain book. Furthermore, he will never believe that the results of his own attempts are final."[15]

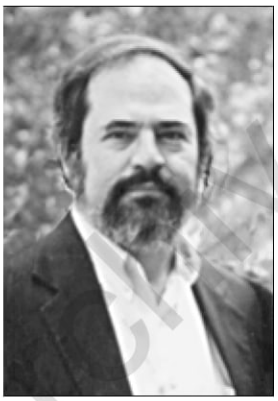
On the other hand, Dr. James DeMeo has unearthed ambiguities in Einstein's reaction to the threatening experimental results from Dr. Dayton C. Miller, who in June 1933 published in *Reviews of Modern Physics*, "The Ether-Drift Experiment and the Determination of the Absolute Motion of the Earth."[16] In the present issue, DeMeo (p. 72) provides an outstanding critique of the Miller work and its apparently glib rejection by others, such as Einstein's biographers, who dismiss Miller's work outright. Though Miller's extensive experimental work is not crucial to Einstein criticism, Einstein's and others' reaction to it is very telling.

Canonization

Ian McCausland, in "Anomalies in the History of Relativity" (p. 19), traces some of the historical reasons for Einstein's rapid rise to dominate the world of physics, following the eclipse observations by Eddington and others in May 1919. These were widely believed to have confirmed Einstein's General Theory of Relativity (1916), which extended the 1905 Special Relativity Theory (SRT) to the realm of gravity and formulated a geometrization of space-time curvature as gravity's "explanation." From *Time* magazine (December 31, 1999, p. 58), this historical truth is acknowledged: "Einstein, hitherto little known, became a global celebrity and was able to sell pictures of himself to journalists and send the money to a charity for war orphans. More than a hundred books were written about relativity within a year."

But as McCausland reveals, the 1919 eclipse observations were flimsy, indeed, and were in no sense a validation of General Relativity. But from that point on, it was impossible to stop the Einstein juggernaut, even in the face of alternative theories to relativity and experimental observations which contradicted it. Today, some physicists seem to believe that Special Relativity has been elevated to the level of *fact*, not theory—criticism of it is neither allowed nor respected. By implication, those who *do* criticize it are foolish incompetents. Witness Caltech Professor David L. Goodstein in his video-taped lecture, "Atoms to Quarks," part of "The Mechanical Universe and Beyond" video physics lecture series (generally an excellent overview of conventionally accepted physics):

> . . .there's a point of view that says that the only way that science can make progress is by showing that theories are wrong. The argument goes like this: It's impossible to prove that a theory is right no matter how many experiments agree with it, but if one single experiment *disagrees* with it, then the theory must be *wrong*. Well, that itself is a theory of knowledge, which is wrong! Because, there are theories in science, which are *so well verified by experience* that they become promoted to the status of *fact*. One example is the Special Theory of Relativity—it's still called a theory for historical reasons, but it is in reality a simple, engineering fact, routinely used in the design of giant machines, like nuclear particle accelerators, which always work perfectly. Another example of that sort of thing is the theory of evolution. These are called theories, but they are in reality among the best established facts in all of human knowledge.



No one who calls himself a scientist should *ever* declare that any *theory* is beyond future revision, even drastic revision, no matter how solid the support for the theory may seem to him. Goodstein has fallen into the trap of so many physicists today: They confuse the apparent mathematical fit of several or many of a theory's descriptive formulae with the right to conclude that the theory must be fundamentally correct and without contradiction. Those *apparent* contradictions that are admitted, are patched over with *ad hoc* arguments to save the epicyclic masterpiece. For example, Special Relativity can't properly deal with extended or rigid bodies (*i.e. real* bodies), though it is seemingly fine for point-particles. See comments about that topic in this issue by Thomas Phipps (p. 37) and William Cantrell (p. 12).

The certainty with which the physics establishment reveres Einstein's relativity theories has become a dominant feature of the intellectual milieu of our age. More examples: A brief passage from Marcia Bartusiak's *Einstein's Unfinished Symphony:*[17] "'The worship of Einstein, it's the only reason we're here [working on an expensive federally funded device, LIGO, to test General Relativity by trying to detect gravity waves], if you want to know the truth,' says Rainer Weiss of MIT. 'There was this incredible genius in our midst, in our own lifetime. . .There's a mystique.'"

Ronald W. Clark, one of Einstein's most illustrious biographers, wrote, ". . .the unqualified acceptance and the experimental verification that had long ago put the Special Theory beyond all dispute were still lacking here [for General Relativity].[18] Special Relativity "beyond all dispute"? Such incautious words.

It is well known that *Time* magazine emblazoned Albert Einstein on its December 31, 1999 cover, designating him "Person of the Century." Inside that issue he was called "first among the century's giants," "its greatest scientific genius," "the person who, for better or worse, personified

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our times and will be recorded in history as having the most lasting significance," "a symbol of all the scientists," "the world's first scientific supercelebrity," "the century's greatest thinker," and even ". . .the patron saint of distracted schoolkids."

Time et al. should have heeded this sentiment by Einstein himself:

> "It strikes me as unfair, and even in bad taste, to select a few individuals for boundless admiration, attributing superhuman powers of mind and character to them. This has been my fate, and the contrast between the popular assessment of my powers and achievements and the reality is simply grotesque." (From a 1921 interview with a Dutch newspaper, reprinted in Reference 15, p. 8.)

Next in line for sainthood in physics has been Stephen Hawking, whose involvement with virtually mystical (unproved but highly mathematized "radiating black holes") has catapulted his *A Brief History of Time* book's sales into the high seven-figure range. In his "Brief History of Relativity" for *Time's* Einstein glorification issue, he declares that Einstein "cut through the ether and solved the speed-of light problem once and for all." Hawking states, "I still get two or three letters a week telling me Einstein was wrong. Nevertheless, the theory of relativity is now completely accepted by the scientific community, and its predictions have been verified in countless applications." This shows that even scientific "saints" such as Hawking, are fallible. Correction for Dr. Hawking: Just as the physics establishment refuses to fairly judge the cold fusion/low-energy nuclear reaction experiments of recent vintage, the historical record back to the turn of the century overflows with relativity-falsifying experiments that are marginalized as "unimportant" —just as no doubt are those Einstein-critical letters which Hawking likely does not read. *Time* magazine's editorializing suggested that Einstein's reputation would endure at least one thousand years. Hawking was much more bold: "The equations of general relativity are his best epitaph and memorial. They should last as long as the universe."

The hyperbolic adulation heaped on Einstein's achievements might have been a hint that something was seriously amiss. Personally, I had been brow-beaten into unquestioning belief in Special Relativity, until in the 1990s I began to question much that is taken for granted by the physics establishment and its army of journalist sychophants. Note these commentaries in books that I have examined over the years:

> ". . .all barriers are surmounted by a superhuman endeavor which up to now has withstood all tests and attacks. This is the story of relativity."(1954)[19]

> "Einstein's special and general theories of relativity have permanently changed our view of space and time and gravitation."(1992)[20]

> "The intellectual culture of our time cannot be fully understood without taking into account the impact of the this theory. Not only electrodynamics of moving bodies, but every physical theory that has been formulated since them has had to confront Einstein's revolutionary changes in notions of space and time." (1992)[21]

Perhaps the best interpreter of this institutionalized arrogance has been Thomas Phipps.[10] This passage from his paper in this issue bears repeating: "Toward the end of his life Einstein remarked that he wouldn't want to be starting over again. He died in 1955, at which time he didn't know the half of it. Had he lived another fifty years, he wouldn't have wanted to start to be starting over. For by that time the character of physics had changed: The Einstein doctrines had been set in concrete and the kind of heretical departure from the status quo that his special relativity theory (SRT) originally represented had become 'dissidence'—despised, ridiculed, and banned from the literature by all properly indoctrinated, right thinking physicists. In that short but fateful interval of time Albert Einstein had become the new Claudius Ptolemy and the little world of professional physics had voluntarily condemned itself to a thousand years of trimming down the great world to fit into a bed of 1905 philosophical truth."

To all this Einstein might have replied with good humor, as he did to a friend in 1930, "To punish me for my contempt of authority, Fate has made me an authority myself."[15]

Conservation of Paradox

There are many reasons to be concerned about the persistence of the Relativity theory's aura of invincibility. It introduced a permanent sense of paradox and confusion about time and its relation to space. (If such paradox were necessary, we could live with it—"philosophical taste" is not the fundamental issue in Relativity criticism.) Relativity allowed such probable fictions as Big Bang cosmology and "black holes" to exist—if there is no space-time, only time and space, then there is no possibility of expanding space and time from a singularity at the "beginning of time." Most important is Einsteinian relativity's totally unwarranted abolition of the aether and all that an aether might imply: just possibly, the aether might be a source of energy; it might be a transmission medium for barely imaginable things; and, horror of horrors, it might have something to do with the functioning of life itself, as Wilhelm Reich had seemed to find in his experiments. These speculations aside, there simply *never* was a good reason for throwing out the aether. It had been the plenum and medium for light waves to *wave*, once in the nineteenth century it began to be more accepted that light did indeed have wave-like properties. Earlier, Isaac Newton had insisted that light consisted of tiny corpuscles, and his arguments had dominated for over a century. Then in the early twentieth century there emerged with the birth of Quantum Mechanics a chimeric version of light as *both* wave and particle. Precisely *what* light is, how it or some essence travels across space, and how it is emitted and absorbed are still matters subject to experiment and debate.

The spirit in which Einstein put forth Special Relativity is best captured in his statement, "Physical concepts are free creations of the human mind and are not, however it may seem, uniquely determined by the physical world." (1938, in a book with his associate Leopold Infeld, *The Evolution of Physics.)* A bad beginning, or so it transpired, to have placed

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a bet on a mental construct without tethering it firmly to the experiments of others. His theory, which (it was *later* said) attempted to explain the experimental record of the late nineteenth century by a novel combination of postulates, was but one of several possible theoretical alternatives that might have preserved *invariance* of physical laws within frames of reference moving at constant relative velocity (see William Cantrell's "Commentary on Maxwell's Equations and Special Relativity," (p. 12). Heinrich Hertz, Hendrick A. Lorentz, and Henri Poincaré had already developed mathematical structures that could have been applied more judiciously over a longer period of time to evolve an appropriate and non-paradoxical theory to deal with the admitted non-invariance of Maxwell's equations. Instead, Einstein with his two postulates made what amounted to an untested, brilliant gamble or guess. He proudly termed it a "free creation of the human mind"—so

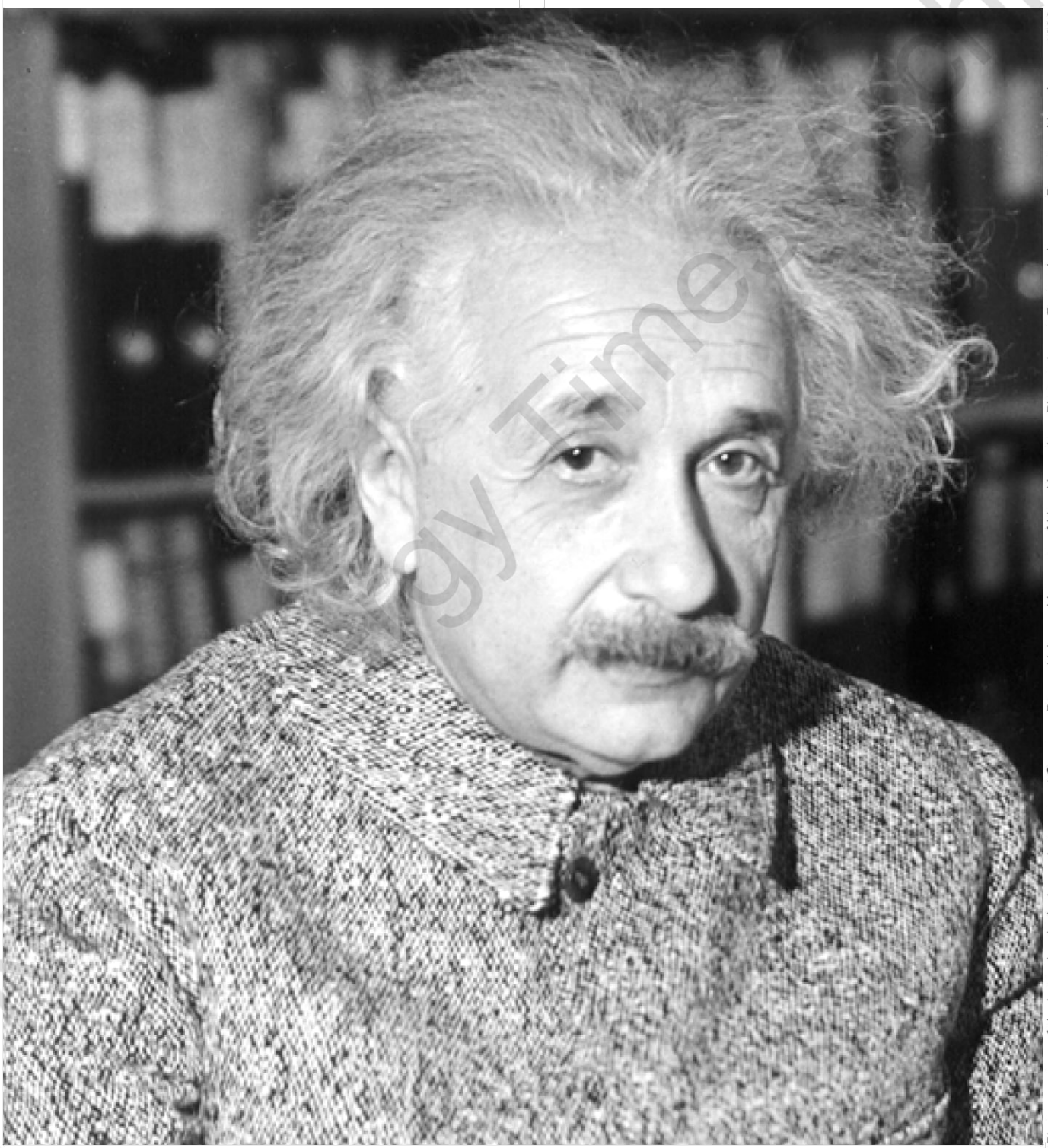

Photo courtesy of the Lotte Jacobi Archives, Instructional Services, Dimond Library, University of New Hampshire. Photo taken at Einstein's home in Princeton, New Jersey, 1938.

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ambiguously connected with past experiment to the extent that historians continue to debate what Einstein knew or did not know of Michelson-Morley *et al.* and when did he know it.[22,23] Einstein's lucky guess applied the sledgehammer of the Lorentz transformation (the multiplying factor $\gamma = (1-v^2/c^2)^{1/2}$), where v is the relative velocity of two inertial frames) to time and space. When the world of physics prematurely latched onto this "ingenious" formalism, the rush-to-judgment bypassed the careful consideration of alternatives.

The several alternatives to SRT, which are by now substantially developed, do no violence to our basic concepts of time and space as distinct entities. As William Cantrell states eloquently: "Einstein's SRT tampers with space and time in order to *force* the speed of light to be constant *with respect to all observers*. And it pays the price. The theory is reminiscent of a balloon animal. If squeezed at one end, it expands at the other, yielding an overall conservation of paradox." And as the Correas point out in their paper in this issue, "Consequence of the Null Result of the Michelson-Morley Experiment: The Demise of the Stationary Aether, The Rise of Special Relativity, and the Heuristic Concept of the Photon" (p. 47), the Albert Michelson-Edward Morley experiment of 1887 (at the Case Institute in Cleveland, Ohio) appeared to rule out a static aether. But certainly, this experiment did not eliminate a *dynamic* aether of some kind that might form something like an "aetherosphere," which was, at least near the surface of our planet, nearly in rotation with it.

It is not the aim of this short editorial space to discourse extensively on the manifold failings of relativity theory, or to detail the alternative theories to SRT which address these. The papers and references we have noted and selected for this issue and the next serve that function well; they are among the best of that technical criticism, but they are just a beginning. (Our apologies to all those other critics of relativity whose excellent work is not showcased.) Our central objective is to show that such criticism does exist, that it is reasoned, and that there have long been open questions about relativity, which have been *deliberately ignored* by the Physics Establishment. We hope that this coverage will inspire those who remain free-thinking and who are not intimidated by the prevailing intellectual tyranny that passes for physics today. We hope especially to reach the uncorrupted—young students of physics who may help pioneer new ways of experiment and understanding.

Yet here lies a central problem and a paradox in its own right. Some critics of Einsteinian relativity have correctly observed that their criticism is weak, because it has lacked a *generally accepted* replacement theory that could satisfy most critics. There are, to be sure, too many competing dissident theories. Thus, the single rallying point of the mainstream, SRT, has triumphed by default. Yet, in striving for a new corrective point of view, one should be cautious not to sanction a new dogma.

The Path Beyond Relativity

There is no question that in the late nineteenth century physics needed to try to find a theory that would extend the *relativity principle* of Newtonian mechanics to optical phenomena and electromagnetism. Einstein's bold, but flawed synthesis seemed to be workable. That its formulae led to excellent quantitative fit in some experimental regimes concealed its all too apparent logical inconsistencies and inability to encompass other experiments. The physics establishment fell in love with the idea that a lone genius, Einstein, had stood on the shoulders of others to come up with the radical synthesis that abolished the aether and conventionally understood space and time. This elevated the profession of physics by establishing an elite group, which boasted that it could comprehend the spatialization of time and the many paradoxes inherent to SRT.

Many may be surprised to learn that the most perceptive critics of Einstein's relativity theories employ rational methods of scientific argument and analysis; they have performed the essential mathematical treatments. It is natural that newcomers may have misgivings about these critics, because they have been bludgeoned with what have been claimed to be iron-clad proofs of SRT predictions, such as length contraction and time dilation. In truth, the experimental record contains *no* proof of length contraction and it has a highly muddied collection of "proofs" of time dilation *per se*. No, the existence of altered decay of subatomic particles such as muons does not prove time dilation, no matter how often that canard is repeated in textbooks (see, for example the critique by Cantrell). Even the famous $E=mc^2$ formulation, supposedly one of Einstein's most original concepts, has alternative derivations, some of which were in an advanced state by the time SRT burst forth onto the scene. And, SRT's famous mass increase with velocity can be viewed quite differently. The infamous "twin paradox" can be abolished. Not only that, there is no reason why advanced space ships could not far exceed light velocity (see Dr. Cynthia Kolb Whitney's papers and conclusions, referenced and summarized on p. 65-66). She notes, ". . .long-distance space travel is seen to be not impossible in principle. We are now limited not by the speed of light, but rather by the speed of thought, which the present author submits is actually infinite when thought is liberated from dogma."

The late Herbert E. Ives of Bell Laboratories, one of the most illustrious of Einstein critics, published one of his many perceptive articles in the *Journal of the Optical Society of America*, "Genesis of the Query, "Is There an Ether?,'"[24] which we have reprinted in this issue (p. 30). In this short piece he appears to shred the illogic of Einstein's second postulate (the supposed requirement for the constancy of the velocity of light measured by all observers), and he defines the false constraints (no use of moving clocks to synchronize other clocks), and other problematic assumptions of Einstein's SRT. No matter —you will be hard-pressed to find mention of Ives' compendious work in any of the biographies of Einstein and books about relativity. Perhaps the well-documented approach of Ives' should become a starting point for relativity criticism (see introduction to Ives' work, (p. 29).

Something Overlooked?

It must be admitted that *most* alternatives to Einstein's relativity theories seem to focus on mathematical alterations to eliminate the theories' inconsistencies with the experimental record. But it now appears probable to this reviewer that something much more profound has been missed by most of

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the critics—the possibility of *new experimental investigations* heretofore overlooked. This new direction is being pioneered by Dr. Paulo and Alexandra Correa, whose laboratory work builds upon the experimental findings of Wilhelm Reich in the middle of the last century and casts their experimental findings in a formalized theory with full mathematical support. (See their newly released materials, available on a new website—www.aetherometry.com—which was to appear shortly after this issue went to press.) Their new publication stream began with their paper in the last issue of *Infinite Energy*,[25] continues with their second paper in this issue, and with another Einstein-related paper to appear in our next issue ("The Sagnac and Michelson-Gale-Pearson Experiments: The Tribulations of General Relativity with Respect to Rotation").

They summarize the essence of their new direction in their current paper: "The authors propose that Einstein's heuristic hypothesis be taken as factual—the result being that electromagnetic radiation becomes secondary to an energy continuum that is neither electromagnetic nor amenable to four-dimensional reduction. It follows that the second principle of SR only applies to photon production, which is always and only a local discontinuity. It does not apply to non-electromagnetic radiation, nor, *a fortiori,* to the propagation of energy responsible for local photon production." *[Editor's note:* Einstein's "second principle" is the postulate of the supposed constancy of light speed *in vacuo* to all observers.]

In another profound assertion, which goes directly to the heart of unraveling the mystery of $E=mc^2$, they state: "We have proposed our own aetherometric analysis of these type of experiments, where it it shown that the experimental velocities of massbound charges are predicted by a theoretical model that does not take recourse to any of the Lorentz transformations. That means—no time dilation and no relativistic mass increase with acceleration of inertial mass. The inertial mass of a system is only a measure of its rest energy, unlike what SR proposes it is." They have contempt for the relativistic and other orthodoxies that presume to have abolished the aether: "Having become the official logico-mathematical theory of physics, relativistic orthodoxy, as much as quantum and wave mechanics, refuses to conceive of any form of energy that is not electromagnetic or associated with mass-energy. To speak of the aether these days only brings smiles of contempt from institutional physicists—they have already found something better: the intangible 'swarming of virtual particles'." So, will the aether return with a vengeance and an Aether Energy Age soon begin? We shall see.

Finally, what shall we take as the most important thing to be learned from the almost century-long Einstein hiatus in physical theory? As with cold fusion and LENR, which is for all practical purposes the return of alchemy—*proved* this time in scientific studies—is that even the most sacrosanct of ideas, Einsteinian Relativity, can be dead wrong. In fact, the late Richard Feynman may have said it best when he identified what he considered to be the most important implication of Relativity, though in the context in which he voiced this, he certainly did not mean that he thought Relativity itself was wrong!:

> What then, *are* the philosophic influences of the theory of relativity? If we limit ourselves to influences in the sense of *what kind of new ideas and suggestions* are made to the physicist by the principle of relativity, we could describe some of them as follows. The first discovery is, essentially that even those ideas which have been held for a very long time and which have been very accurately verified might be wrong. . .we now have a much more humble point of view of our physical laws—everything *can* be wrong![26]

As regards physics of the late twentieth century and early twenty-first, Feynman (a.k.a. "Genius," so-called by author James Gleick) was profoundly wrong about the "humble" part. But indeed, almost everything *was* wrong, and we must begin anew to correct it, with arduous experiment and new theory.

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Commentary on Maxwell's Equations and Special Relativity Theory

William H. Cantrell*

Abstract

The importance of Einstein's Special Relativity Theory (SRT) is discussed in the context of our modern technology and the progress of science. Historical reasons are given for the development of SRT, and the problems it attempted to solve concerning The Holy Grail of Science: *Maxwell's Equations*. (See definitions of technical terms in display boxes, p. 13, 14). The justification for SRT, that of making Maxwell's Equations covariant to inertial translation by using the Lorentz-transformation, is discussed. This, in turn, creates problems, paradoxes, and logical flaws, which are enumerated herein. SRT is challenged by at least three alternative theories from various researchers. These theories merit attention because they do *not* require the concept of length and time to be modified (tampered with) to obtain correct answers.

To admit a fundamental mistake in such a hyped-up theory would be an embarrassment, not only to the physics community at large, but also to the memory of a man whose portrait hangs in nearly every physics department around the world.

Introduction

There can be no doubt that Einstein's Special Relativity Theory (SRT) has had a profound impact on the physics of the twentieth century. His theory has enjoyed a series of brilliant successes for some ninety-five years now. Yet there is a sizeable community of scientists who *reject* it outright. And there is a far larger group who harbor a pronounced distaste for the theory, but know of no feasible alternatives to it. This widespread dislike stems from the fact that Einstein's theory is codified in the Lorentz transformation, and this transformation tampers with two foundational concepts of physics, length, and time measurement, in order to *force* the speed of light to be constant for all observers. When thoughtful, skeptical people are asked to abandon time-honored definitions of space and time, they sense a logical flaw, a sleight of hand. Given a clear alternative to SRT, these mavericks would *jump* at the chance for a substitute theory. Of course, there are the vast majority of engineers and scientists who have no need for SRT, for it plays no role in their profession. At best, it has a specialist role to play in a few branches. At worst, it may have stifled creativity and retarded the rational development of science.[1]

So why has Einstein's theory remained popular for so many years? I believe there are at least five reasons. First, the alternative theories have never been given much attention nor taught at any university. Second, the establishmentarians have invested a lifetime of learning in maintaining the *status quo*, and they will act to protect their investment.[2] (For the readers of this magazine, there is no need to elaborate on this point.) Third, Einstein's theory, being rather vaguely defined and self-contradictory by its own construction, allows some practitioners to display an aura of elitism and hubris in their ability to manipulate it. There is an exclusive quality to the theory—like a country club, and that is part of its allure. Fourth, to admit a fundamental mistake in such a hyped-up theory would be an embarrassment, not only to the physics community at large, but also to the memory of a man whose portrait hangs in nearly every physics department around the world. And fifth, contrary to popular myth, Einstein was very good at *public relations*. During the Great Depression, the popular-culture was actively seeking "heroes" from all walks of life to bolster public morale. Einstein actively courted the press as the stereotypical brainy scientist. In response, Hollywood *loved* it and promoted the image—and *that* helps to explain the many portraits.

The Holy Grail of Science

Some of today's "popular" books on science imply that the most important theory to come out of the modern age is Einstein's Relativity. This perception is incorrect. Go back in time to the Age of Steam, and then imagine the *giant* technological leaps about to occur—the creation of the electric light, the telegraph, the telephone, and that most magical of inventions, the *wireless*. Sparkling with mystery and intrigue, its very name was completely descriptive. It captivated and tantalized the world's imagination like nothing else. Indeed, it must seem difficult to believe that the invention of radio took on an atmosphere of excitement and *glamour* exceeding that of the Internet and the communications revolution of today.

Radio, computers, and in fact the whole foundation for our electro-technical civilization is canonized in *Maxwell's Equations*. These equations, published in "rough form" in 1864 by James Clerk Maxwell,[3] were based upon the pioneering experimental work of Coulomb, Ampère, Faraday, and others. The theoretical genius that he was, Maxwell developed a unifying set of equations for electricity and magnetism. From "The Maxwell Equations," as they are sometimes called, the wave equation can be derived for electromagnetics. This equation predicted the existence of radio waves, and it fell to the theoretical and experimental genius, Heinrich Hertz, to prove their existence in 1888. Make no mistake about it, the Maxwell Equations are *The Holy Grail of Science*, second in importance only to the work of Sir Isaac Newton.

So where does SRT fit into the picture? Well, it turns out

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that there is a problem with the Maxwell Equations. The hallmark of a good theory is that it is *invariant* to the Galilean transformation. Unfortunately, the Maxwell Equations are not invariant to this transformation. As a matter of fact, they're *not* invariant to the Lorentz transformation either. (They are *co*variant to the Lorentz transformation. Like most branches of science, electrodynamics has its own jargon. See below and the following page insets for some helpful definitions, especially that of invariance versus covariance. *Most textbooks get this wrong.*)

So the dilemma facing late nineteenth century physicists was that of a very successful set of equations with an obvious flaw. The equations successfully predicted radio waves. They pointed Hertz toward their discovery in his laboratory. They were (and are) simple, elegant, and beautiful in the mathematical sense. Yet they flunked a most important test, that of Galilean invariance. A scientific crisis had erupted. What to do?

Special Relativity Theory

Enter Einstein in 1905 with his famous paper on SRT.[4] Contrary to popular belief, he was not attempting to address the 1887 Michelson-Morley (M-M) null-result, and was reportedly *unaware* of it at the time.[5] In effect, he borrowed, and then redefined, the Lorentz transformation in order to address the invariance problem with the Maxwell Equations. He also stated two postulates.

Einstein's First Postulate was a restatement of the *principle of relativity* (not to be confused with the *Theory* of Relativity). The principle of relativity is the anti-Ptolemaic epitome of enlightenment and common sense, and was known during the time of Galileo, if not earlier. It was first stated by Newton in his *Principia*[6] over three hundred years ago. In essence, it says that *the laws of physics, when properly formulated, remain equally valid in all (inertial) frames moving with uniform velocity with respect to each other.* All physicists accept this principle without reservation.

Einstein's Second Postulate states that the velocity of light is independent of the state of motion of its emitting source. Actually, this is not at all an unusual proposal, and it would be expected of a medium-based (aether) theory. The analogy to sound waves is irresistible. When a train whistle blows, the speed of sound is independent of the speed of the train, but *not* of the velocity of the wind carrying the sound to the observer. Here, the air molecules are the medium, and they play the equivalent role of the aether for electromagnetism. But SRT is *not* an aether-based theory, at least not *directly.* (General relativity uses the concept of "curved space-time." Matter supposedly causes the curving of "space," and "curved space" causes the bending of light rays. From a metaphysical standpoint, curved space-time is just as audacious and arbitrary as the nebulous aether of the nineteenth century.) And although not explicitly stated by Einstein, some thought will convince the reader that the Second Postulate, when combined with the principle of relativity, results in a velocity of light that is also independent of the receiver (the observer), because the emitting source can take on any velocity including that of the receiver. So here we have a prediction resulting in a sharp departure from our everyday experience.

It has been stated that Einstein independently *derived* the Lorentz transformation using his two postulates, but this is not true. Taken alone, they are insufficient to derive the Lorentz transformation *uniquely*. Further assumptions must be made to do so; otherwise, several alternative transformations can be derived from them.[7] Nevertheless, in 1905 this was a tentative start toward a salvage operation of the Maxwell Equations for observations at high speeds, and physicists were willing to overlook the smell of covariance, and the use of an *ad hoc* transformation, if it would rescue the Holy Grail. As Phipps has pointed out,[8] this was a bizarre turn of events, for there *were* competing theories at the time, theories capable of remedying the situation without the need to tamper with the time-honored concepts of space and time. This is all the more poignant, considering the fact that the Lorentz transformation intermixes and scrambles space and time, something that is unfounded and erroneous.

Problems and Paradoxes of SRT

Before we present alternative theories that could be used to *replace* SRT, it is important to discuss the serious problems and paradoxes associated with the *use* of SRT. The full impact

What is a moving inertial frame?

With respect to a stationary coordinate system (usually a Cartesian coordinate system in the laboratory), we define a *second* coordinate system in uniform motion at constant velocity. For simplicity, this second reference frame is normally aligned with the laboratory in *x, y,* and *z,* and has its direction of motion parallel to the *x*-axis. This moving reference frame does not speed up or slow down, nor does it rotate. A typical application would be to place a moving electric- or magnetic-field detector in this second reference frame.

What is a Galilean-transformation?

Between moving frames of reference, time is unchanged and velocities add linearly as expected. Galileo explained that a stone dropped from the crow's nest of a ship would strike the deck at the same distance from the mast as viewed from shore, even though it appeared to drop vertically aboard the ship and [along a parabolic trajectory] from ashore. The transformation equations are shown below for *any* velocity *v* along the *x*-axis:

$$x' = x - vt$$
$$t' = t$$

What is a Lorentz-transformation?

There are no examples from everyday experience to rely upon. Both time and length are changed by this transformation, and the two become intertwined. The transformation equations are shown below for a velocity $v < c$ along the *x*-axis:

$$x' = \gamma(x - vt) = \frac{x}{\sqrt{1-\frac{v^2}{c^2}}} - \frac{vt}{\sqrt{1-\frac{v^2}{c^2}}}$$

$$t' = \gamma\left(t - \frac{vx}{c^2}\right) = \frac{t}{\sqrt{1-\frac{v^2}{c^2}}} - \frac{vx}{\sqrt{c^4 - v^2c^2}}$$

$$\text{where } \gamma = \frac{1}{\sqrt{1-\left(\frac{v}{c}\right)^2}}$$

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of this most unusual theory is rarely discussed, and the first warning signs of trouble appeared shortly after the 1905 paper was published. For simplicity, a single Lorentz transformation is normally applied along one coordinate axis. But what about the more general case of *two* linear, but non-aligned (non-collinear) translations? You might assume that this merely adds complexity to the calculation, but not so—the theory breaks down. The results depend upon the *order* in which the two translations are applied. For example, apply two transformations in succession, with the velocities of the two systems pointed in different directions, say, along the *x*-axis and then along the *y*-axis. Next, repeat the calculation by reversing the order, *e.g.*, the *y*-axis and *then* the *x*-axis. The result is different. Hence the Lorentz transformation fails to obey the commutative law of mathematics, (a+b = b+a) and a definitive answer eludes us. This is a paradox, an absurdity.

Thomas Rotation

Theoreticians were brought in to resuscitate SRT and they administered the so-called "Thomas Rotation" ("Thomas Precession," when applied to the electron). This is a rotation of coordinate axes introduced to compensate for the error between the two results mentioned above. It might seem a bit odd that an inertial system, that is, a moving system with fixed velocity and no acceleration nor rotation, *should begin to rotate!* Furthermore, where did this rotational energy come from? Is this a real effect, or is it some sort of mathematical artifact, some indication that the Lorentz transformation (and hence SRT) is in error?

Some years later, a theoretical prediction[9] for Thomas Rotation was published in the British journal *Nature*, and an experiment was conducted by Phipps to determine whether it really existed. The experiment produced a null result, and despite the photographic evidence, it was refused publication in *Nature*. Consequently, the results were published elsewhere.[10,11] There is also a theoretical basis for refutation of the Thomas Rotation.[12]

The Ehrenfest Paradox

If phantom rotation wasn't bad enough, the next disease to afflict SRT was the rigid body problem, otherwise known as the "Ehrenfest Paradox." Einstein had originally intended SRT to apply to rigid bodies only. Ehrenfest asked the question, "What happens when we have an idealized rigid disk, and we set it into rotation?"[13] The outer edge of the disk can be divided into infinitesimal segments such that they appear to be moving with a linear velocity with respect to an observer in the laboratory. How would the Lorentz contraction affect such an object, as viewed by the observer? The radius must contract somehow. In 1910, the first generally accepted answer (now called the Herglotz-Noether theorem) said that, since the disk was rigid, *it could not rotate*! Of course, there were those who decried this sort of non-solution solution, so the next answer to bubble to the surface said the disk *could* rotate if it was somewhat *elastic*, and *not* made of rigid material. So once again, we have a radical departure from everyday experience. Newtonian mechanics has absolutely no problem with rigid spinning bodies, while Einstein's approach expressly forbids them on purely mathematical grounds.

The Lorentz Contraction

There is also the pole-vaulter paradox, the lever paradox, and a host of other variants. What these all have in common is the Lorentz contraction. But do atomic particles shrink in the direction of their motion, as viewed by an external observer? Do macro-objects really shrink? The author of the Lorentz transformation considered the contraction effect to apply *only* to a deformable electron based on an aether-stress theory.[14] He never intended for his mathematical-equivalence contraction formula to be extrapolated *to all matter* as a reality. This might explain why H.A. Lorentz was adamantly opposed to SRT until his death in 1928. He *intended* it to explain the electron's non-uniform concentration (bunching) of electric field lines perpendicular to its direction of acceleration. In electrical engineering, this is similar to the "skin-effect" at macroscopic levels.[15, pp.149-151] But length contraction plays no role.

During the first half of the twentieth century, physicists were eager to put the Lorentz contraction to the test, and see if the phenomenon really existed. Several experiments were performed,[16-18] but no variation in length was observed. Recently, a modern space-based test has been proposed by Renshaw.[19] To date, *no **direct** experimental verification of relativistic length contraction has ever been measured.*

What is invariance?

An equation (or set of equations) is said to be *invariant* under a particular transformation of its independent variables, if the form of each term of the equation is unaltered by the transformation.[8, p.116.] Obviously "Lorentz-invariance" is an oxymoron.

What is covariance?

An equation (or set of equations) is said to be *covariant* under a particular transformation of its independent variables, if the form of each term of the equation is not left unaltered. That is to say, each term is altered by the transformation in the same manner.[8, p.118.] For the case of the Lorentz-transform, this is best illustrated by quoting H. Minkowski of four-vector fame: *"Henceforth space by itself, and time by itself, are doomed to fade away into mere shadows."* (Space and time are scrambled—like eggs.)

What are the Hertzian-modified Maxwell Equations?

Shown here are the equations (using rationalized MKSA units) in free-space, where the constitutive equations have been used to replace *D* and *H* with *E* and *B*:

$$\nabla \cdot \boldsymbol{E} = \rho / \varepsilon_0$$

$$\nabla \cdot \boldsymbol{B} = 0$$

$$\nabla \times \boldsymbol{E} = -\left\{ \frac{\partial \boldsymbol{B}}{\partial t} + \boldsymbol{v} \cdot \nabla(\boldsymbol{B}) \right\}$$

$$\nabla \times \boldsymbol{B} = \mu_0 \boldsymbol{J} + \frac{1}{c^2} \left\{ \frac{\partial \boldsymbol{E}}{\partial t} + \boldsymbol{v} \cdot \nabla(\boldsymbol{E}) \right\}$$

$$\nabla \cdot \boldsymbol{J} = -\left\{ \frac{\partial \rho}{\partial t} + \boldsymbol{v} \cdot \nabla(\rho) \right\}$$

The Continuity equation has been added at the bottom. Here the partial derivatives ($\partial/\partial t$) in Faraday's and Ampère's Laws have been replaced with time derivatives (d/dt), and expanded using the chain-rule. Hence, a new velocity term, *v*, appears for use with moving reference frames. These equations are *invariant* to the Galilean transformation.[8]

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The Twin-Paradox

The twin-paradox (also known as the clock-paradox) is without a doubt the most famous paradox associated with SRT. Once upon a time, there were two twin brothers. One twin ventured into outer space at relativistic speeds, while the other twin stayed home. As extrapolated by SRT, the Lorentz transformation causes time itself to slow down, not just for moving subatomic particles, but for atoms and molecules, and for bigger objects, say, *people*!

To continue with the story, the space traveling twin eventually *reverses* course to return to Earth. Upon his arrival, he discovers that his Earth-bound brother has aged many years, while he has aged only a few. This is considered to be a paradox because each twin (each observer) can claim that it is the *other* who moves at high speed as viewed in his own reference frame. So in SRT, how can one age more than the other? The symmetry-breaking event is alleged to be the fact that the space traveling twin must reverse course in order to return. This causes him to undergo accelerations and decelerations that the other does not experience. No clear explanation is given as to why this would break symmetry and slow the aging process, especially over a many year period, where the actual time involved in acceleration could be quite small.

. . .There is no time-dilation. This myth is perhaps the greatest extrapolation of elementary observation in the history of science.

In reality, there is little doubt that both twins would age at the *same* rate. So there really is no paradox because there is no time-dilation. This myth is perhaps the greatest extrapolation of elementary observation in the history of science. Let's take a careful look at the evidence supporting the time-dilation aspects of SRT.

Time-Dilation

The alleged proof for time-dilation is claimed to be among the most confirmed experiments in physics. Yet a careful dissection of these experiments reveals an equally plausible alternative explanation, one that does not require time to be a dependent variable.[20,21] There are three types of experiments that address this issue, the rate of radioactive decay of high-speed mesons in linear motion[22] and in circular orbit,[23] the transport of atomic clocks around the globe,[24,25] and (indirectly via) the relativistic Doppler formula.[20] As Beckmann has pointed out, in all cases the experimenters have failed to ask, let alone answer, whether *time itself* is dilated, or whether internal processes are simply slowed by moving through a gravitational field.[15, pp.77-81] To date, *no **direct** experimental verification of relativistic time-dilation has ever been measured.*

Do we know what the "innards" of a high-speed meson consist of? Nope. Do we know what causes natural radioactive decay? Not really. It can be characterized mathematically by a Poisson distribution, but the actual internal "trigger" for a particular decay is unknown. Could the rate of decay be affected by angular accelerations, or by traversing a gravitational potential? Think about the last time you rode an amusement park ride. Weren't *you* affected?

In 1761, the British Royal Navy awarded John Harrison a cash prize of 9,000 £ (over $2,000,000 in today's currency) for inventing a navigation-quality timepiece with enough accuracy to withstand the pitching and rolling seas of the Atlantic. The magnitude of the residual error has diminished over the centuries, but the basic problem remains. We cannot construct an *ideal* clock using actual materials, even if we use cesium atoms *by definition*. To emphasize this point, let's take a look at a grandfather clock. If we transport such a clock eastward around the globe, it will slow down. But if we transport it westward, it will *speed up*. The grandfather clock relies upon the force of gravity to control the timekeeping rhythm of its pendulum, in inverse proportion to the square root of *"g."* When transported westward *against* the rotation of the earth, the centrifugal force of the earth's rotation is diminished and the effect of its gravity field is strengthened, if ever so slightly. Hence the clock speeds up in an increased gravitational field. And of course, if flown eastward *with* the earth's rotation, its centrifugal force is strengthened and the clock slows down a little. Time-dilation? Of course not. We explain the outcome of the experiment by analyzing the "innards" of the clock. But the influence of gravity applies to more than just pendulum clocks. For example in the famous Häfele-Keating experiment,[25] the atomic clock transported eastward lost 59 ns, but the atomic clock transported westward *gained* 273 ns, compared to the stationary laboratory standard. All physical devices used for time keeping are subject to error when accelerated, decelerated, or constrained to move linearly through a variation in gravitational potential.

So if we can't rely upon experiment, let's do the next best thing and look to theory for an answer. What does electrodynamics have to say? Oleg Jefimenko (of Generalizations of Coulomb and Biot-Savart laws[26] fame) has answered this question most eloquently. Using conventional electrodynamic theory,[27] he has analyzed the interactions of charged particles. The simple arrangement shown in Figure 1 can be used as an oscillatory particle-clock. In many cases, these "particle-clocks" behave precisely as predicted by SRT, apparently a brilliant and stunning con-

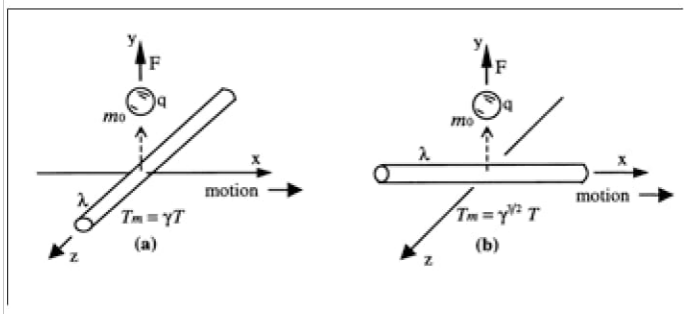

Figure 1. A Jefimenko charged-particle clock. Charged particle "q" is constrained to bounce up and down vertically, along the *y*-axis (repelled upward by a like-polarity line-charge "λ"). This forms a simple oscillatory clock with period *T* at rest, and period *Tm* in motion. When the charges are moved along the *x*-axis as shown, the clock shown in (**a**) obeys the Einstein time-dilation rule, $Tm = \gamma T$, where $\gamma = 1/\sqrt{1-(v/c)^2}$.

When oriented 90 degrees as shown in (**b**) and moved along the *x*-axis, the same clock does *not* obey this rule. Here $Tm = \gamma^{3/2}\ T$.

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firmation of the theory. But when this same particle clock is oriented 90 degrees to its direction of motion, it behaves differently. This is not predicted by SRT (apparently a shocking and stunning defeat?). In the case of Jefimenko's particle-clocks, they slow down for purely conventional reasons, having nothing to do with time-dilation. Some clocks slow in accordance with the Lorentz transformation and *some do not*. But SRT adopts the Lorentz transformation exactly, without any wiggle room. In the final analysis, the concept and definition of *time* is metaphysical. It ought not to be subjected to the whims of anyone's pet theory.

Alternative Theories

Einstein's SRT tampers with space and time in order to *force* the speed of light to be constant *with respect to all observers*. And it pays the price. The theory is reminiscent of a balloon animal. If squeezed at one end, it expands at the other, yielding an overall conservation of paradox. At least five alternatives to SRT have been proposed:

Physics, if it is to understand the real world, must build on the two primitive and undefinable pillars [space and time]. It must not tamper with them in order to accommodate higher concepts. It must not redefine the unthinkable; more particularly, it must not make the primitive pillars observer-dependent.

1. The speed of light is constant with respect to an unentrained (or *partially* entrained) aether. (This is the equivalent of an absolute reference frame and violates the *principle* of relativity.)
2. The speed of light is constant with respect to the emitting source. This is the Ritzian "ballistic" theory where the speed of light is (v+c), like projectiles fired from a moving tank (and the antithesis of the Second Postulate).
3. The speed of light is constant with respect to a *fully* entrained ether.
4. The speed of light is constant with respect to the dominant local or *gravitational* field, as proposed more recently by Beckmann.[15]
5. The speed of light is constant with respect to the absorber (the detector), as proposed more recently by Phipps.[8]

Long ago, the great optical experimentalist, Albert A. Michelson, disproved the first two of these five theories. Note that the M-M experiment was compatible with *all* of these theories with the exception of number 1. In a separate experiment, Michelson showed conclusively that number 2 was untenable.[28] So contrary to popular myth, Michelson believed that he had actually *confirmed* the existence of the aether, via theory number 3.

Beckmann has noted that number 3 and number 4 are nearly equivalent theories, if you replace the outdated term "aether" with his more radical idea of "gravity." Beckmann's theory (number 4) squares with all of the experimental evidence, because in every case, the observer has always been tied to the Earth-bound frame of reference. (The double-star evidence does *not* refute his theory.) The light emitted (from binary stars revolving about a common center of mass) would indeed travel with two different velocities initially, and this would cause spectral anomalies that are not observed. But the light rays would merge to a common velocity as the gravitational fields from the two stars merged into one dominant field.[15, p.37.] And the dominant field would change yet again upon encountering an observer in the Earth's gravitational field. Beckmann's theory and SRT predict the same answers to first-order in (v/c), and a decisive experiment would have to be performed at second-order, $(v/c)^2$.

In his outstanding book,[8] Phipps argues forcefully and confidently for his absorber theory (number 5) by starting with some of the original ideas of Heinrich Hertz. Hertz proposed a minor modification to the Maxwell Equations in order to make them invariant to the Galilean transformation. The modification involved a simple and straightforward change from partial derivatives $(\partial/\partial t)$ to time derivatives (d/dt) in Faraday's and Ampère's Laws. This had the excellent effect of adding a velocity parameter to the Maxwell Equations (a very pragmatic modification for dealing with moving inertial frames, wouldn't you say?). But unfortunately, Hertz assigned the *wrong* definition to his new velocity term, one having to do with the fashionable aether wind of the nineteenth century. As a result, his "Hertzian Theory" was shot down by experiment. To make matters worse, his untimely death in 1894 from blood poisoning, at the age of thirty-six, made it impossible for him to catch his mistake and reassign proper meaning to the new parameter.

Phipps has continued this work by assigning the velocity of the detector to this Hertzian parameter. Phipps goes on to propose an experiment which can decide between SRT and his theory. Furthermore, his experiment can determine the victor at *first-order* in (v/c). This sheds some light on why experiment has not yet determined a winner among the various theories mentioned. With the available technology, experiments at first-order are extremely difficult to do, and experiments at second-order are impossible.

In view of the problems with SRT and the availability of alternatives, is it any wonder that there were (and are) several noted authorities who would have nothing to do with SRT? Some of the famous ones included Dingle, Essen, Ives, Mach, Russell, and Rutherford. *(My apologies to anyone I may have missed (or included) in the* Who's Who Directory of Heretical Physics.) Michelson was even rumored to have said, *"I have created a monster."* Wheeler and Feynman also toyed with the idea of an acausal absorber theory to replace SRT. And late in life, Einstein was said to have had second thoughts.

It is interesting to note that the speed of light is approximately the same as the escape velocity from an idealized electron in a Bohr orbit.[29] Perhaps it will turn out that theories number 2, 4, and 5 are *all* correct within their respective gravitational spheres of influence.

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Faster than Light?

There is a new crop of experimental evidence favoring superluminal velocities. Radio waves have been reported to travel faster than "c," at least initially, in the laboratory.[30] However, this result has been challenged as experimental error.[31] Velocities of up to "10c" transverse to line of site have been reported by astronomers. More evidence has surfaced that the speed of light is not "c" in deep space, based on data from the Pioneer 10 and 11 satellites.[32]

Recently, physicists at Princeton made an announcement indicating that they have broken the light barrier.[33] One researcher was quoted as saying: "Our experiment does show that the generally held misconception that 'nothing can travel faster than the speed of light' is wrong." However, their results have been challenged as experimental error due to anomalous dispersion. (Recall that non-TEM transmission modes in waveguide or in optical fibers are subject to dispersion, even when the medium is *lossless*. This spreads out an energy pulse, giving the illusion of increased speed at the leading edge.)

Another heresy to modern physics is the dissident assumption that gravity must act *instantaneously* ($\geq 10^8 c$) in order to explain the motions of the heavens.[34] If gravity propagated (slowly) at "c," the effects of aberration would cause the Earth's orbital energy to increase, and spiral slowly outward. NASA is said to use an instantaneous velocity for the speed of gravity in its calculations.

"Recreational Mathematics" and Its Impact to Physics

I have witnessed dissident protests in the classroom (see *IE* No. 33, pp. 8-9). No doubt the politics of Einstein's Relativity have contributed to the malaise within the physics community. In a recent editorial,[35] a noted Emeritus professor of physics discussed the situation with regard to students leaving academia for more challenging work in industry. He goes on to say: "We are experiencing a serious brain drain in physics. . .those who are leaving are in many cases the cream of the crop. I hear this again and again from many different mentors. I find many of those hired for permanent jobs in physics [academia] to be among the least creative. . .my impression is that [industry] is doing a better job in pinpointing creativity. . .[Our profession] believes that there are no more scientific revolutions possible. . .We must not let *The End of Science* become a self-fulfilling prophecy. The best way to prevent the end of science is to provide opportunity in abundance for the most creative and original of our young people. This is not happening. But it needs to."

The mavericks have often made the point that, once cut loose from the constraints of experiment, modern physics has drifted into the realm of abstract, untestable mathematics. G.H. Hardy would have been very pleased. Quoting Beckmann: "Mathematics is perfectly free and unfettered by experimental observation to define its axioms from which it deduces their consequences; physics, if it is to understand the real world, must build on the two primitive and undefinable pillars [space and time]. It must not tamper with them in order to accommodate higher concepts. It must not redefine the unthinkable; more particularly, it must not make the primitive pillars observer-dependent."

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New Energy Companies and Inventors
Status Reports on Technologies and Claims

This is a continuing series of updates on the activities and achievements of new energy companies, organizations and inventors.

DW Energy Research, LLC
1350 Northface Court, Colorado Springs, Colorado 80919
Phone: 719-598-1426 (David Wallman)

We first reported on the carbon-arc process of Wilbur Dammann and David Wallman in 1996. We spoke to Wallman recently for an update on their technology and corporate activity. DW Energy Research, LLC has designed and constructed laboratory-grade research and development units. They have also made progress in efficiency improvement by applying the Le Chatelier Principle in chemistry. The result has been several new patents. In particular, U.S. Pat. No. 6,113,065 (Reactor Configuration For a Liquid Gasification Process) reveals the use of a spherical reactor chamber with a reflective surface. Wallman stated, "We determined that it was more important to contain the UV light from the carbon-arc and concentrate it at the point of the arc, than it was to harness the light energy externally. Since the reaction to produce Carbo-Hydrogen™ gas (COH_2) is endothermic, this increases the production rate and the efficiency of the process." Wallman further stated that "efficiency has increased to 12 ft^3/kilowatt-hour of electrical input. Testing has yielded an energy value of 550 BTU/ft^3; this equates to 6600 BTU of gas energy per kilowatt-hour of electrical input, which represents an energy amplification factor of about 1.9!

DW Energy Research also discovered information that indicates why the gas exhibits more heating value than should be possible from a 550 BTU/ft^3 gas. Wallman received information from Dr. William A. Rhodes, an applied physicist, the inventor of "OxyHyd" gas, commonly referred to as "Brown's" gas. The following is the communication from Dr. Rhodes to Wallman:

SUBJECT: Messages from William A. Rhodes
on Carbon-Arc Gasification Process & Paper - July 1999

From a volume in my library, *Essentials of General Chemistry*, Hopkins & Bailar, University of Illinois, 1946, page 57, Atomic Hydrogen Torch, "At extremely high temperatures, hydrogen molecules are broken into single atoms; these recombine immediately upon cooling, with liberation of 38,000 calories of heat per gram. Use is made of this in the atomic hydrogen torch (Figure 42), in which molecular hydrogen is dissociated into atoms in an electric arc. As it leaves the arc, the atomic hydrogen recombines and also burns, so the amount of heat liberated in the flame is enormous; much higher temperatures can be reached in this way than with the ordinary oxyhydrogen torch."

Have you considered what was said in one of my previous e-mails, where a carbon arc torch was fed tank hydrogen that was broken into atomic gases, recombines as atomic, releasing one level of energy, then burns as molecular, doubling temperature? BTU is increased. How such a pattern might fit in your system should be considered.

Re: Atomic Hydrogen Torch, *Essentials of General Chemistry*

Further calculations of 38,000 calories of heat per gram suggests you may be generating atomic hydrogen from the arc which is returned as additional heat (70.5 BTU/min) from recombination of H_1 to H_2. In this case arc-heating of water is also not considered. Therefore, pages 8/9 BTU calculations (in your COFE paper) are assumed correct with all energy accounted for.

When converted to BTUs, 38,000 calories per gram equates to approximately 150 BTU/ounce of gas.

Wallman also said they have explored several variations of biomass solutions with favorable results. We will continue to monitor developments of the Biomass Gasification Process.

See *Infinite Energy* No. 10

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Anomalies in the History of Relativity

Ian McCausland*

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Abstract

In November 1919, it was announced to the world that observations of a solar eclipse that occurred in May 1919 supported Albert Einstein's general theory of relativity. That announcement was one of the most influential events of twentieth century science, since Einstein's instant rise to enormous fame arose directly from it. In spite of the confidence with which the announcement was made, however, it was later realized that the accuracy of the observations was insufficient to constitute a reliable confirmation of the phenomenon that was predicted. Furthermore, another of the formulas published in the general theory, for the variation in the perihelion of the planet Mercury, had already been derived by another scientist several years earlier using another method. In spite of the fact that the experimental evidence for relativity seems to have been very flimsy in 1919, Einstein's enormous fame has remained intact and his theory has ever since been held to be one of the highest achievements of human thought. The resulting deification of Einstein has had some unfortunate effects: critics of his theory are often dismissed as cranks, and the search for better theories has been inhibited. It is suggested that the announcement of the eclipse observations in 1919 was not a triumph of science as it is often portrayed, but rather an obstacle to objective consideration of alternatives.

Introduction

The generally accepted version of the history of relativity has been dominated for about eighty years by the famous announcement in November 1919 that the results of observations at a solar eclipse in May 1919 confirmed Albert Einstein's prediction from his general theory of relativity that starlight would be bent by a precise amount by the gravitational field of the Sun. The announcement caused such an enormous sensation, both among scientists and the general public, that Einstein and his theory became world famous, literally overnight. In spite of the confident announcement, however, it was later realized that the observations were not particularly accurate and that the results did not in fact provide decisive support for the theory. An interesting commentary on the situation is found in the first two paragraphs of an article by the Editor of *Nature* (Maddox, 1995), which are as follows:

> Historically, the most dramatic proof that Einstein was on the right track with his general theory of relativity came in 1919, when Eddington and his associate Crommelin took themselves off to the western Pacific and the track of the total eclipse of the Sun in that year. They were bent on measuring the predicted deflection of light from distant stars lying near the limb of the Sun. As the world knows, the prediction was confirmed within the limits of the accuracy of the observations, to general applause. Einstein became a celebrity overnight.
>
> What is not so well remembered is that the measurements in 1919 were not particularly accurate. The standard error of the measurements was roughly 30%, while the displacement of the images of different stars in the field near the eclipsed Sun ranged from half to twice the deflection Einstein had predicted. And later eclipse measurements have not been much better, largely because of the difficulties of making accurate measurements from the *ad hoc* observatories that have to be established.

That quotation shows the two conflicting sides of the problem: Einstein's enormous and enduring fame resulted directly from the announcement of the eclipse results, although the results were not particularly accurate. Another interesting description of the problem is given by Sciama (1967):

> The first attempt to test Einstein's prediction was made by British expeditions led by Sir Arthur Eddington just at the end of the Great War. At that time it was considered very striking that a group of British astronomers should attempt to test a theory proposed by a German. The results of the test were claimed as a triumph for Einstein, and his worldwide fame dates from this event. Ironically enough, attempts made at later eclipses suggest that Eddington underestimated the uncertainties inherent in such a difficult observation, and even today Einstein's prediction has not been tested with the precision one would wish.

The purpose of the present paper is to present more details of the history of the period in question and to attempt to assess the significance of the announcement of the 1919 eclipse observations.

The General Theory of Relativity

The general theory of relativity (Einstein, 1916) gave a formula that correctly matched the anomalous advance of

In spite of the fact that the experimental evidence for relativity seems to have been very flimsy in 1919, Einstein's enormous fame has remained intact and his theory has ever since been held to be one of the highest achievements of human thought.

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the perihelion of the planet Mercury, which had already been observed by astronomers such as Le Verrier (Roseveare, 1982). The general theory also made two further predictions: that light would be bent by a gravitational field, and that spectral lines of light from stars (including the Sun) would appear displaced towards the red end of the spectrum because of the strong gravitational fields.

The predicted effects of gravitation on light were so small that they caused no measurable phenomena on the Earth itself, but the theory predicted that light reaching the Earth from a star would be bent by the gravitational field of the Sun if the light passed very close to the Sun. Since a star whose light passed very close to the Sun would usually be impossible to see, the deflection of starlight by the Sun is very difficult to observe. However, such deflections might be observable during a total eclipse of the Sun, and in March 1917 the Astronomer Royal of Britain, Sir Frank Watson Dyson, suggested that the total eclipse of the Sun that was to take place on May 29, 1919, would present an excellent opportunity to test this prediction of the general theory (Dyson, 1917). Although 1917 was at a dark period of the first world war, Dyson undertook the organization of the eclipse expeditions in the hope that the war would be over in time for the expeditions to take place. Even though the war did in fact end in time for the expeditions, Dyson did not go to observe the eclipse himself. Instead, Professor Arthur Stanley Eddington became the leader of the eclipse expeditions, and the subsequent rise of the general theory was strongly influenced by Eddington's work. (As a minor comment on the above quotation from Sciama, it should be pointed out that Eddington was knighted in 1930, so he was not Sir Arthur when he went on the eclipse expeditions.)

Although it appears that Dyson was originally somewhat skeptical about the possibility that Einstein's general theory would be confirmed (Eddington, 1939-1941), Eddington was a strong believer in the theory and was confident that it would be confirmed. Chandrasekhar (1987) has described Eddington's influence on Dyson by writing:

> Eddington's enthusiasm for the general theory of relativity must have succeeded in ensnaring into its magic his close friend and associate Sir Frank Dyson, the Astronomer Royal; for together, they were soon planning expeditions to observe the solar eclipse of May 29, 1919 if, to quote Jeans, "the state of civilization should permit when the time came." Eddington has described his own part in the planning and in the successful outcome of the expeditions "as the most exciting event, I recall, in my connection with astronomy."

Chandrasekhar went on to say that Eddington was so confident of the correctness of the theory that, if left to himself, he would not even have planned to go on the eclipse expedition. Chandrasekhar related how Eddington, a pacifist, was deferred from military service in the first world war "with the express stipulation that if the war should end by May 1919, then Eddington should undertake to lead an expedition for the purpose of verifying Einstein's prediction!"

The Eclipse Expeditions and Their Observations

Two expeditions went from Britain to observe the total eclipse. Professor Eddington and Mr. E.T. Cottingham went to the island of Principe, in the Gulf of Guinea, West Africa, while Dr. A.C.D. Crommelin, and Mr. C. Davidson, went to Sobral in Brazil. Neither expedition went to the western Pacific as reported by Maddox (1995). Photographic plates of the stars close to the Sun, obtained during the eclipse, were compared with plates taken of the same portion of the sky at a different time of year when the Sun was elsewhere.

It should be noted that the deflection predicted by Einstein (1916), was very small, being about 1.7 seconds of arc for a ray of light at the edge of the Sun, and inversely proportional to the radial distance from the center of the Sun. Since no stars for which measurements were made during the 1919 eclipse were within two solar radii of the center, the largest deflection that would have occurred according to the theory would have been less than 0.8 seconds of arc, which, for the 343 cm focal length of the telescope used by Eddington, would have corresponded to about 0.01 mm on the photograph. In addition, there were other technical difficulties because of having to transport telescopes to remote locations. For example, light from the stars was reflected by a mirror into the telescope, so that the mirror could be rotated to compensate for the rotation of the Earth during a time exposure, instead of rotating the telescope, which was not feasible under the conditions of the eclipse expeditions.

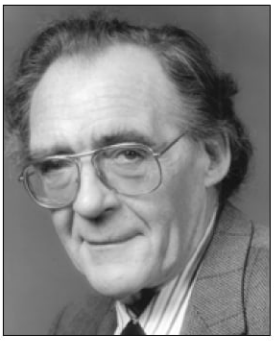
Possible sources of error in observations of this type at solar eclipses have been described by various authors such as von Klüber (1960) and Bertotti, Brill, and Krotkov (1962). Some of these are:

1. Refraction of light in the Sun's corona and/or in the Earth's atmosphere.
2. Distortions in the optical system caused by temperature changes during the eclipse.
3. Changes of scale between the eclipse plates and the comparison plates.
4. Distortions in the photographic emulsion while drying.
5. Errors in measurement of the images on the plates.

Although refraction in the Sun's corona was probably not significant, refraction in the Earth's atmosphere may have been. For example, here is a recent estimate of the effect of the atmosphere on astronomical observations (MacRobert, 1995):

> Viewed at high power from the bottom of our ocean of air, a star is a living thing. It jumps, quivers, and ripples tirelessly, or swells into a ball of steady fuzz. Rare is the night (at most sites) when any telescope, no matter how large its aperture or perfect its optics, can resolve details finer than 1 arc second. More typical at ordinary locations is 2- or 3-arc-second seeing, or worse.

Although that article was in the "Backyard Astronomy" section of the journal, it seems reasonable to suggest that the conditions at Sobral and Principe in 1919 were probably not as good as the conditions obtainable in an American backyard observatory in 1995. The Sobral telescopes were set up on a race-course, and the set-up is shown in photographs in various publications such as Eddington (1920). The author is not aware of any published photographs of the Principe set-up, but it may be noted that the baggage had to be transported


File Photo

John Maddox, former editor of *Nature Magazine*—no friend of heretical physics—nonetheless harbors some doubts about general relativity.

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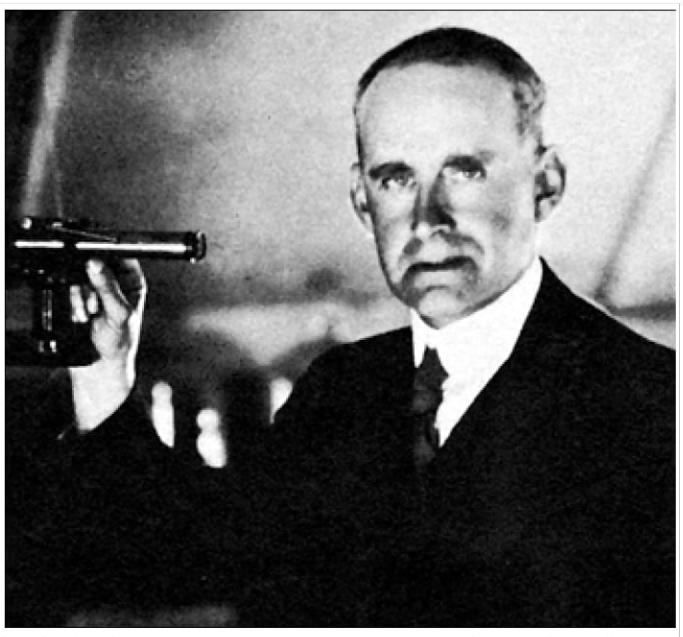

Sir Arthur Stanley Eddington. His observation of the solar eclipse of May 29, 1919 was largely responsible for the explosive rise of Einstein's fame.

about a kilometer through the woods by native carriers (Dyson *et al.*, 1920), so the set-up was unlikely to have been any better than at Sobral. In addition to the ordinary "everyday effects of the "ocean of air", the darkening of the Sun during the eclipse may itself have worsened the refractive effects.

Distortion in the optical system may have been caused by the effect of the heating of the mirror by the Sun, and this was noted as a problem at Sobral (Dyson *et al.,* 1920).

In considering changes of scale between the eclipse plates and the comparison plates, von Klüber (1960) has pointed out that, for a telescope with a focal length of 343 cm, a change in the effective focal length or focal setting of only 0.1 mm, between the eclipse plates and the comparison plates (which are taken months apart), can cause scale errors of the same order as the Einstein effect for stars beyond about 8 solar radii from the center of the Sun.

Distortion of the emulsion while drying was probably not very significant. However, measurements of the actual deflections on the plates may have introduced some errors in view of the extremely small displacements involved, especially when the x and y components of each displacement have to be measured.

In addition to all the above potential problems, the weather was cloudy at Principe at the time of the eclipse, and the photographs obtained were not as good as expected. In one of the earliest published references to the findings of the eclipse expeditions, the Astronomer Royal, Sir Frank Dyson, reported as follows at a meeting of the Royal Astronomical Society on July 11, 1919 (Fowler, 1919), before Eddington had returned from the expedition:

> I had a letter from Prof. Eddington two days ago. He is hoping to get good enough measures to determine the displacement definitely, but he obviously is greatly disappointed. He secured sixteen photographs, but only for the last six the sky was clear enough to show any stars, and on them he only got three, four, or five images; and, as the sky was generally only clear on one part of the plate at a time, the stars secured on the plates are badly distributed. From his best plate, however, he has some evidence of deflection in the Einstein sense, but the plate errors have yet to be fully determined.

It is interesting to compare that reference to Eddington's own later description of his measurement of that same plate. He describes how, although the weather was unfavorable because of clouds, sixteen photographs were taken. Of the twelve that were examined at Principe, most were useless, but, "one plate was found showing fairly good images of five stars, which were suitable for a determination." (Eddington, 1920, p. 115.) His description of this plate (p. 116) is as follows:

> The results from this plate gave a definite displacement, in good accordance with Einstein's theory and disagreeing with the Newtonian prediction. Although the material was very meagre compared with what had been hoped for, the writer (who it must be admitted was not altogether unbiased) believed it convincing.

Eddington's biographer, Professor A. Vibert Douglas (1957), has also described his measurement of that plate, quoting from Eddington's Notebook. In the Notebook entry for June 3, after saying that he spent the whole day measuring, Eddington continued as follows:

> The cloudy weather upset my plans and I had to treat the measures in a different way from what I intended, consequently I have not been able to make any preliminary announcement of the result. But the one plate that I measured gave a result agreeing with Einstein.

Douglas' biography continued with the statement that, "This was a moment which Eddington never forgot. On one occasion in later years he referred to it as the greatest moment of his life!"

It is interesting that the measurements on the same plate should have been viewed in such vastly different ways by Eddington at different stages of his study of the findings of the expedition: at first he was obviously greatly disappointed, then (although admittedly biased) he believed the evidence convincing in spite of the meager results, and later he saw it as the greatest moment of his life! Although he had at first suggested (in his letter to Dyson) that the stars on the plates were poorly distributed, he claimed in the published paper (Dyson *et al.*, 1920) that the five stars shown on the two plates from Principe "include all the most essential stars," and even suggested that the cloud may have been beneficial by preventing over-exposure of the brightest stars! In spite of that optimistic assessment, it appears from the published paper that the stars on the two usable plates obtained by the Principe expedition were indeed poorly distributed. Since the expected deflections, according to Einstein's theory, were radial, it seems obvious that a good test would include stars all around the Sun; but on the diagram showing the stars (Dyson *et al.*, 1920, p. 294), it is possible to draw a straight line through the center of the Sun in such a way that all the stars on both plates are well to one side of the line. In fact, it is possible to draw a straight line through the center of the Sun in such a way that, of the thirteen stars included in the combined records of both expeditions, only one (#2) is on the other side of the line. Furthermore, all five stars on one of the Principe plates (plate W, stars # 3, 4, 5, 6, 10), and four of the five on the other (plate X, the one measured by Eddington at Principe, stars # 3, 4, 5, 6, 11), are almost on a

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straight line. Since the measurements on the Principe plates were taken by placing a Principe plate film to film with an Oxford comparison plate so that the images of corresponding stars nearly coincided (which was possible because the Oxford plates were taken direct and the Principe plates by reflection in a mirror), the almost straight-line distribution of stars may have worsened the accuracy of fitting together the pairs of plates. (A diagram of the stars can also be found in Eddington, 1920, p. 120.)

By contrast, the Sobral expedition had experienced fine weather; it had taken photographs using two telescopes, one similar to the Principe one and another having a different aperture and focal length. The photographs were also disappointing because the definition of the images had been spoiled. When the photographs taken by the telescope similar to the Principe one were measured, "The measures pointed with all too good agreement to the 'half-deflection,' that is to say, the Newtonian value which is one-half the amount required by Einstein's theory." (Eddington, 1920, p. 117.)

Eddington (1920) went on to give a reason for preferring the Principe results: the astronomers at Principe had taken "check-plates" of another portion of the sky to check whether there was any effect of the 50-degree difference in temperature between Principe in May, and England in January, when the photographs were taken for comparison, whereas no such check-plates were taken by the Sobral expedition. However, the Sobral expedition had stayed in Brazil for a further two months after the eclipse (Eddington, 1920, p. 117) to photograph the same region of the sky before dawn, so it would appear that the large temperature difference would not apply to the comparison of the photographs in that case. The published paper (Dyson *et al.*, 1920) states why it was not feasible for the Principe expedition to make a similar comparison:

> Unlike the Sobral expedition, we were not able to take comparison photographs of the eclipse field at Principe, because for us the eclipse occurred in the afternoon, and it would be many months before the field could be photographed in the same position in the sky before dawn. The check plates were therefore specially important for us.

In any event, it seems strange to state after the fact that the absence of check-plates of the Brazil expedition was a disadvantage. Either those check-plates were significant or they were not; if it was considered necessary to take them, that should have been decided *in advance* rather than being used *after the fact* to downgrade or reject a whole set of observations. A somewhat different opinion of the importance of the check-plates has been given by A.I. Miller (1996, pp. 88-89):

> Although the weather was far better at Sobral, Eddington insisted on emphasizing data from Principe. Clearly, a key part of this experiment is to use the same instruments at the same site for comparing data for the apparent and true star positions, but this was not done. The comparison photographs were taken several months later at Oxford.

There is also a puzzling discrepancy between the two accounts: although Eddington (1920, p. 117) states explicitly that "there were no check-plates taken at Sobral," the other account (Dyson *et al.*, 1920) states on p. 298, referring to the Sobral expedition, that "A few check plates of the field near Arcturus were taken, but have not been used."

Several authors have referred to the way in which Eddington emphasized some photographic plates rather than others. For example, Campbell (1923) wrote as follows:

> Professor Eddington was inclined to assign considerable weight to the African determination, but, as the few images on his small number of astrographic plates were not as good as those on the astrographic plates secured in Brazil, and the results from the latter were given almost negligible weight, the logic of the situation does not seem entirely clear.

As a minor addition to the history of the eclipse expeditions, it may also be noted that, at the same meeting at which Dyson reported Eddington's disappointment (Fowler, 1919), Campbell reported on the results obtained at an earlier eclipse on June 8, 1918, by saying, "It is my own opinion that Dr. Curtis's results preclude the larger Einstein effect, but not the smaller amount expected according to the original Einstein hypothesis." (This refers to an earlier version of Einstein's prediction of the displacement, which was half the amount predicted by the general theory.)

Even after the measurements have been made, there is still the mathematical problem of extrapolating back to the edge of the Sun, since no stars measured were within two solar radii of the center of the Sun and small changes in the observed deflections can cause a much greater change in the computed deflection at the limb of the Sun. This problem has been discussed by von Klüber (1960) and Bertotti, Brill, and Krotkov (1962); in particular, Figure 6 of von Klüber's paper shows extremely widely scattered results from various eclipse expeditions. A more recent eclipse expedition in 1973 has been described by Zirker (1995), who shows widely scattered measurements (Figure 9.4, p. 179) and states that the occurrence of a sandstorm limited the value of the results.

The problems of interpreting the results of the eclipse expeditions have been well summarized by Guggenheimer (1925), who made the following comment on the 1919 eclipse observations:

> Any reader, though far from an expert astronomer or physicist, who will study the description of the apparatus used in these observations and the large margin of error possible by reason of defects therein, will readily comprehend that, in view of the required delicacy of measurement of the things observed and of the error allowances both for apparatus defects and other possible physical causes of the observed phenomena, the greatest caution in the analysis of the results is necessary. Some astronomers deny that the photographs of the eclipse observations, when compared with those taken of the same stars in the absence of the Sun, show deflections approximating the amount or the direction predicted by Professor Einstein. An examination of the various tables of the deflections observed shows that many of them are far away from the quantities predicted. The quantity approximating the predicted one is obtained by averaging a selected few of the observations.

Announcement of the Eclipse Results

The results obtained by the British eclipse expeditions of May 1919 were announced at the famous joint meeting of the Royal Society and the Royal Astronomical Society held on November 6, 1919. In spite of the poor accuracy and the uncertainties surrounding the results, it was announced that the evidence was decisively in favor of the value of dis-

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placement that had been predicted by Einstein, and Sir Joseph Thomson, President of the Royal Society and Chair of the meeting, strongly endorsed the results (Thomson, 1919) The enthusiastic reception of the announcement at the meeting was matched by an enormously strong reaction by the general public and, according to one biography of Einstein (Clark, 1971), "Einstein awoke in Berlin on the morning of November 7, 1919, to find himself famous." Although Folsing (1997, p. 448) has expressed disagreement with that particular interpretation of the event, there is little doubt that Einstein rose with spectacular rapidity to worldwide fame as a direct result of the announcement.

The atmosphere of excitement at the meeting has been described by Alfred North Whitehead (1926) in the following words:

> It was my good fortune to be present at the meeting of the Royal Society in London when the Astronomer Royal for England announced that the photographic plates of the famous eclipse, as measured by his colleagues in Greenwich Observatory, had verified the prediction of Einstein that rays of light are bent as they pass in the neighborhood of the sun. The whole atmosphere of tense interest was exactly that of the Greek drama: we were the chorus commenting on the decree of destiny as disclosed in the development of a supreme incident. There was dramatic quality in the very staging: the traditional ceremonial, and in the background the picture of Newton to remind us that the greatest of scientific generalizations was now, after more than two centuries, to receive its first modification. Nor was the personal interest wanting: a great adventure in thought had at length come safe to shore.

The meeting has also been described in a very interesting way by Abraham Pais (1982), who identified the day of the joint meeting as "the day on which Einstein was canonized." Pais was obviously very pleased with his comparison of the meeting to a Congregation of Rites at which a candidate is considered for canonization in the Catholic Church, and compared various participants at the meeting to counterparts in the Congregation of Rites, using as his reference *The New Catholic Encyclopedia*. However, one of his comparisons was highly inappropriate, namely his comparison of Ludwik Silberstein to the *advocates diaboli*, or Devil's advocate. Although Silberstein was critical of accepting that the results superseded Newton's theory, the circumstances in which his criticisms were expressed were totally different, in at least two important respects, from the circumstances in which the arguments of the Devil's advocate are expressed in a Congregation of Rites.

In the first place, it is imperative that the arguments of the Devil's advocate be heard *before* canonization is pronounced, and Pais' account gives the impression that Silberstein's criticisms had been heard before the results were endorsed by the President of the Royal Society. However, the generally accepted account of the meeting (Thomson, 1919) shows that Thomson had, to use Pais' words, "pronounced the canonization" before Silberstein had had a chance to speak.

In the second place, it is the responsibility of the Devil's advocate to ensure that canonization does not occur undeservedly. In order to fulfill that responsibility, he must be given full access to all the relevant information required to make the case for the opposition to canonization. This condition was not fulfilled at the meeting at which the eclipse results were announced, because it was not possible for members of the audience to be sufficiently well-informed about the results that were being announced to make informed criticism of them. The paper that was eventually published (Dyson *et al.*, 1920), which is forty-three pages long, with copious tables of results, and mathematical analysis, carries the notation "Received October 30,—Read November 6, 1919." Also, the issue of *Nature* dated October 30 carried an announcement of the joint meeting, showing that the meeting had been arranged before the paper had been received by the Royal Society. It seems very unlikely, therefore, that the paper had received any critical review by independent referees before presentation, or that its contents were available to the audience early enough to be thoroughly studied. Just after the astronomers had presented their results, Thomson rose to call for discussion, but before the discussion actually started, he strongly endorsed the confirmation of Einstein's prediction by saying (Thomson, 1919):

> It is difficult for the audience to weigh fully the meaning of the figures that have been put before us, but the Astronomer Royal and Prof. Eddington have studied the material carefully, and they regard the evidence as decisively in favour of the larger value of the displacement. This is the most important result obtained in connection with the theory of gravitation since Newton's day, and it is fitting that it should be announced at a meeting of the Society so closely connected with him . . . If it is sustained that Einstein's reasoning holds good—and it has survived two very severe tests in connection with the perihelion of Mercury and the present eclipse—then it is the result of one of the highest achievements of human thought.

It was that speech that Pais interpreted as the pronouncement of the canonization of Einstein; the remark about "one of the highest achievements of human thought" has been widely quoted and has obviously contributed enormously to the veneration of Einstein and relativity. If we pursue Pais' comparison of the joint meeting and a Congregation of Rites, we find another unfortunate feature of the comparison, namely that canonization of a saint by the Pope is infallible and irrevocable, so that subsequent criticism of the process is futile.

It is unfortunate that Pais, by identifying Silberstein as the Devil's advocate, gave the impression that a critical assessment of the eclipse results had been voiced at the joint meeting, and that the criticisms had been answered. Let us now consider what Silberstein did say in his contribution to the discussion at that meeting (Thomson, 1919). Although he could not find fault in the eclipse observations, for the reasons given above, he did point out the fact that the third main prediction of the general theory, the red-shift of light in a strong gravitational field, had not been observed. He said:

> There is a deflection of the light rays, but it does not prove Einstein's theory; it cannot be logically deduced from his theory as a gravitational effect in the absence of the spectroscopic result. And, as far as we know from St. John's and Evershed's observations, the predicted shift of the spectrum lines, of the amount exceeding almost 100 times the probable error of the modern spectroscope (as Prof. Fowler has just told us), is not obtained. . .If the shift remains unproved as at present the whole theory collapses, and the phenomenon just observed by the astronomers remains a fact awaiting to be accounted for in a different way.

Eddington's reply to Silberstein was described by Earman and Glymour (1980) as being "mild and irrelevant." He said:

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> When a result that has been forecasted is obtained, we naturally ask what part of the theory exactly does it confirm. In this case it is Einstein's *law* of gravitation. It is not necessarily his theory that is confirmed, with the underlying assumption that *ds* is a quantity measurable by clock and scale. There still remains the question what the intermediary quantity *ds* is, which must be tested by the Fraunhofer lines. [Italics in the original.]

On the question of whether the eclipse results confirmed Einstein's general *theory* or only his *law* of gravitation, the published paper seems to be a little ambiguous; on p. 292 of the published paper (Dyson *et al.*, 1920) the following two statements occur within the course of a single paragraph (emphasis added):

> The results of the observations here described appear to point quite definitely to the third alternative, and confirm Einstein's generalized relativity *theory*. But, whether or not changes are needed in other parts of the theory, it appears now to be established that Einstein's *law* of gravitation gives the true deviations from the Newtonian law both for the relatively slow-moving planet Mercury and for the fast-moving waves of light.

If there is to be a fair comparison between the critical assessment of a scientific paper and the critical assessment of a candidate for canonization, as performed by a Devil's advocate, all available facilities should be placed at the disposal of the assessor in each case. Although Silberstein did not have enough information about the eclipse observations at the meeting itself, he made the following criticisms of them at a meeting of the Royal Astronomical Society on December 12, 1919 (Fowler, 1920):

> They [the eclipse observations] indicate the presence of other factors modifying the displacement of the stars. These displacements are not radial. The deviations from the radial direction are marked running up to 15° for star number 6, and to 35° for number 11. Five of the stars near the Sun's axis show deviations from the radial displacement in the same sense; two stars near the Sun's equatorial plane show angular displacements in the opposite sense. If we had not the prejudice of Einstein's theory we should not say that the figures strongly indicated a radial law of displacement.

It seems reasonable to suggest that the prestige of Dyson and Eddington led to the arranging of the joint meeting before the paper had even been received, and to the rapid acceptance of the paper when it was received. In this connection, it is interesting to note the comment by Wali (1984, p. 115), about the influence of prestige on meetings of the Royal Astronomical Society, that, "Papers had to be submitted a week before, by the first Friday of the month. Papers submitted by people like Eddington, Jeans, and Milne, were always read, and they always came first."

If the experiment supports the theory, the theory is confirmed; if the experiment does not support the theory, it is the difficulties of the observations that are to blame, not the theory.

Although it seems obvious that no independent critical assessment of the results was made before the joint meeting, with information still available today it is possible to make some assessment of the results. As various authors such as von Klüber (1960) and Bertotti, Brill, and Krotkov (1962) have pointed out, other scientists have re-assessed the results of the 1919, and other eclipse results, with a certain amount of variation from the results originally published. For example, Sciama (1969) refers to a table of results of eclipse observations from 1919 to 1952, and comments as follows:

> It is hard to assess their significance, since other astronomers have derived different results from a re-discussion of the same material. Moreover, one might suspect that if the observers did not know what value they were "supposed" to obtain, their published results might vary over a greater range than they actually do; there are several cases in astronomy where knowing the "right" answer has led to observed results later shown to be beyond the power of the apparatus to detect.

Hermann Bondi, a strong supporter of relativity, makes the following comment (Bondi, 1960) on the subject of Einstein's prediction of the deflection of starlight by the Sun:

> Einstein's prediction can therefore be checked only on the rare occasions when, at the moment of an eclipse, bright stars happen to be near the direction to the Sun. The effect is difficult to study even when circumstances are most favourable. The indications are much in favour of Einstein's theory of relativity, but it would be premature to call the results conclusive.

If it was premature in 1960 to call the results conclusive, it was much more so in 1919.

Another interesting assessment of the significance of the 1919 eclipse results has been given by Calder (1979, p. 103), who wrote:

> The eclipse results were a triumph. Newton's ideas about gravity had reigned unchallenged for more than two centuries, yet within four years of Einstein developing his theory it seemed confirmed, and Newton was dethroned. The deflection of light by gravity is, as I have stressed, central to Einstein's general relativity. But later measurements of the deflection of starlight at other eclipses gave a wide scatter of results. They departed from Einstein's prediction by anything up to sixty per cent. The difficulties of the observations were to blame, rather than any defect in the theory. While they did not allow any restoration of Newton they left, nevertheless, a little elbow room for alternative accounts of gravity. So, sixty years after the initial triumph, astronomers and relativists were decidedly cool about this way of checking up on Einstein.

With reasoning like that, the theory cannot lose: if the experiment supports the theory, the theory is confirmed; if the experiment does not support the theory, it is the difficulties of the observations that are to blame, not the theory.

From reading the many accounts of the way in which the results were obtained, it appears that the evidence for the bending of starlight by the Sun was rather flimsy in 1919. Let us now consider another of the results of the general theory, namely its explanation of the movement of the perihelion of the planet Mercury.

The Perihelion of Mercury

As already mentioned, the general theory of relativity

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included a formula that correctly matched the variation in the perihelion of the planet Mercury. Pais (1982) has given the following interesting description of Einstein's excitement on discovering his explanation of that phenomenon:

> This discovery was, I believe, by far the strongest emotional experience in Einstein's scientific life, perhaps in all his life. Nature had spoken to him. He had to be right.

Polkinghorne (1998) quoted Pais' statement and made the following comment:

> It was a great triumph but, if the answer had not come out right, the aesthetic power of the equations of general relativity would have been quite unable in itself to save them from abandonment. It was indeed *nature* that had spoken. [Italics in the original.]

It should be pointed out that exactly the same formula as Einstein published in his general theory for the variation in the perihelion of Mercury (Einstein, 1916) had been derived by another scientist several years earlier using another method based on the assumption of a retarded gravitational potential (Gerber, 1898). Some of the reactions to Gerber's paper have been described by Folsing (1997). In particular, Folsing described the action of physicist Ernst Gehrcke, who had written articles against relativity since 1911, as follows:

> As no one believed his "refutations," Gehrcke in 1916 opened a second front. He unearthed some studies by the Pomeranian schoolmaster Paul Gerber, who, about the turn of the century, had tried to explain the perihelion movement of Mercury by procedures which were entirely arbitrary and were rightly rejected by astronomers. By reprinting Gerber's paper along with his own comment in *Annalen* Gehrcke insinuated, first, that Einstein was a plagiarist and, second, that the problem of the perihelion precession had been solved before relativity theory.

According to Folsing, Einstein did not publish a reply to Gehrcke's paper, but he did publish a brief note (Einstein, 1918) in reply to another paper by Gehrcke (1918); however, although in that paper Gehrcke had mentioned Gerber's formula, Einstein's reply, which Folsing described as "concise and factual," did not mention Gerber. Einstein did, however, mention him in a later article (Einstein, 1920), in which, after agreeing that Gerber had given the correct formula for the perihelion motion of Mercury before he himself had done, Einstein continued as follows:

> The experts are not only in agreement that Gerber's derivation is wrong through and through, but the formula cannot be obtained as a consequence of the main assumption made by Gerber. Mr. Gerber's work is therefore completely useless, an unsuccessful and erroneous theoretical attempt. I maintain that the theory of general relativity has provided the first real explanation of the perihelion motion of Mercury. I have not mentioned the work by Gerber originally, because I did not know it when I wrote my work on the perihelion motion of Mercury; even if I had been aware of it, I would not have had any reason to mention it.

In the same article, Einstein referred to criticism of the eclipse observations as follows:

> Mr. Gehrke in his lecture has made the masterfully executed English measurements of the deflection of light by the sun appear in a bad light by mentioning the three independent groups only one of which, because of aberration of the heliograph mirror, gave erroneous results. He has suppressed the fact that the English astronomers themselves in their official report considered the results as a brilliant confirmation of the theory of general relativity.

In view of the many subsequent criticisms of the eclipse results, it appears that the confirmation of the theory may not have been as brilliant as those who obtained the results believed. It also seems unfortunate that Einstein based his claim of Gerber's errors on an appeal to the authority of "the experts" instead of indicating what was wrong with his derivation.

Another dismissal of Gerber's work is mentioned by Folsing in the following words:

> A sharper, crushing reproof came from the respected Munich astronomer Hugo Ritter von Seeliger, who pointed out that the Pomeranian schoolmaster had copied things which had long been known to every worker in the field, and that his so-called explanation was based on a crude mathematical mistake.

Unfortunately, Seeliger (1918) does not seem to have identified the crude mistake, and Roseveare (1982) has made the following comment on his reply: "Seeliger claimed that Gerber's calculation was based on an elementary mistake, though it is evident that it was Seeliger who was mistaken." Unfortunately, also, Folsing does not say why Gerber's methods, as he put it, "were entirely arbitrary and were rightly rejected by astronomers." It is also unfortunate that Gerber, having died sometime between 1902 and 1917 (Roseveare, 1982), was not available to defend his work from Einstein and Seeliger.

Since Gerber's 1898 paper and Gehrcke's references to it are in German, British scientists may not even have been aware of them in 1919, so that Gerber's work was not taken into account in considering possible alternatives to Einstein's theory. Simply pointing out Gerber's earlier derivation of the formula in question is not, of course, accusing Einstein of plagiarism, since Gerber's derivation of the formula was different from Einstein's.

Although Roseveare (1982) gives a sympathetic description of Gerber's work, he states that his law was refuted, on two counts. The first count was the fact that Gerber's theory is a gravitational theory and says nothing outside that sphere; this problem is apparently avoided by general relativity because, according to Roseveare, "this theory already contains special relativity." That claim is debatable at least, and we shall argue below, in our discussion of special relativity, that special and general relativity are distinct theories; neither one contains the other. Roseveare continues his claim that Gerber's theory has been refuted by saying: "The second count on which Gerber's law may be seen to be refuted is the deflection of light rays, though this was at times rather equivocal." In view of the above discussion of the accuracy of the light-deflection results, this reason does not appear to be decisive. Perhaps it might be interesting to make an objective reappraisal of Gerber's work.

The Special Theory of Relativity

Although the above discussion concentrates on the general theory of relativity, there are also some anomalies in the history of the special theory. When the special theory was first published (Einstein, 1905), it was taken to be similar to theories of others such as Lorentz, Poincaré, and Larmor. For example, Whittaker (1953) mentions Einstein's special theory in passing in a chapter titled; "The Relativity Theory of Poincaré, and Lorentz," in the following words:

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> In the autumn of the same year, in the same volume of the *Annalen der Physik* as his paper on the Brownian motion, Einstein published a paper which set forth the relativity theory of Poincaré and Lorentz with some amplifications, and which attracted much attention.

Although the special theory did not have a very large impact before the rapid rise of the general theory in 1919, the events of 1919 caused it to become famous also, even though it is a different theory. Here is what Cohen (1985, p. 405) wrote about the way the events of 1919 contributed to the rise of the special theory:

> In considering relativity, we must keep in mind that there are two different theories of relativity. . .But what brought the attention of the world to special relativity was the verification in 1919 of a prediction of the general theory, that starlight passing near the Sun is bent by the Sun's gravity. This verification, which occurred during a solar eclipse expedition, set into being the relativity craze that swept the world and overnight made Einstein a public figure.

It is time to realize that, even if the general theory were verified, that does not in itself verify the special theory. The two theories are different; they do not use the same set of axioms. The difference is shown by the following statement by Stachel (1995):

> "Theory of relativity" is an umbrella term, covering two quite distinct theories, usually called "the special," and "the general" theory. The exact nature of the relation between the two is still a subject of controversy, and both might well not bear the common appellation "relativity" if not so closely associated with the work of one man, Albert Einstein, who developed the second in attempting to extend the first to include gravitation.

Discussion

Various authors have tried to explain why the results of the eclipse observations were announced as being decisive when they were not. For example, Earman and Glymour (1980) write that:

> Eddington's overenthusiastic advocacy may perhaps be explained by his prior conviction that the theory was true and by his interest in saving something from the vast work of the Principe expedition. Dyson's position might be understood in part as the result of a reasonable evaluation of the evidence, and in part as the result of Eddington's advocacy. But one retains the suspicion that besides these reasons, there was, especially for Eddington, another: the hope that a British verification of Einstein's theory would force on British scientists a more open-minded and generous attitude towards their German colleagues.

. . . The greatest scorn of the scientific community is reserved for those who would try to criticize either of Einstein's theories of relativity or to suggest alternative theories, and many mainstream scientific journals reject papers critical of either theory without review.

The enormous public reaction to the scientific announcement appears to have been strongly influenced by the ending of the war with Germany, and it may be noted that another important historical event of 1919 was the signing of the Treaty of Versailles, over German protest, in June. Chandrasekhar (1987) has described how Rutherford attributed Einstein's great fame to Eddington and explained the public reaction in the following words:

> The war had just ended; and the complacency of the Victorian and the Edwardian times had been shattered. The people felt that all their values and all their ideals had lost their bearings. Now, suddenly, they learnt that an astronomical prediction by a German scientist had been confirmed by expeditions to Brazil and West Africa and, indeed, prepared for already during the war, by British astronomers. Astronomy had always appealed to public imagination; and an astronomical discovery, transcending worldly strife, struck a responsive chord. The meeting of the Royal Society, at which the results of the British expeditions were reported, was headlined in all the British papers: and the typhoon of publicity crossed the Atlantic. From that point on, the American press played Einstein to the maximum.

It may be argued that the inaccuracies in the 1919 eclipse observations do not matter, since the same effect has supposedly been verified by more accurate measurements such as, for example, the measurements on quasars described by Maddox (1995). This point of view is somewhat guardedly espoused by the last paragraph of the critical discussion of the 1919 eclipse expeditions by Earman and Glymour (1980):

> This curious sequence of reasons might be cause enough for despair on the part of those who see in science a model of objectivity and rationality. That mood should be lightened by the reflection that the theory in which Eddington placed his faith because he thought it beautiful and profound—and, possibly, because he thought that it would be best for the world if it were true—this theory, so far as we know, still holds the truth about space, time and gravity.

That is at least debatable. For example, Chandrasekhar (1990) made the following comments on a lecture in which P.A.M. Dirac had claimed that there was a long and impressive list of successes of Einstein's theory of gravitation:

> It does not seem to me that the successes of Einstein's theory are either long or impressive. It is true that his prediction of the different rates of clocks in locations of differing gravity, his prediction of the deflection of light when traversing a gravitational field and resulting time delay, his prediction regarding the precession of the perihelion of Mercury, and finally, the slowing down of a binary star in an eccentric orbit by virtue of the emission of gravitational radiation, have been confirmed quantitatively. But all these relate to the departures from Newtonian theory by a few parts in a million; and of no more than three or four parameters in a post-Newtonian expansion of Einstein's field equations. And so far, no predictions of general relativity in the limit of strong gravitational fields have received any confirmation; nor are they likely in the foreseeable future.

It is also reasonable to ask whether the rapid and strong entrenchment of the general theory that occurred as a result of the eclipse announcement may have led experi-

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menters to try to obtain the "right" answers from their observations, as suggested in the above quotation from Sciama (1969). For example, Collins and Pinch (1993) have commented on the measurements of the red-shift predicted by general relativity as follows:

> The experimental observations, conducted both before and after 1919, were even more inconclusive. Yet after the interpretation of the eclipse observations had come firmly down on the side of Einstein, scientists suddenly began to see confirmation of the red-shift prediction where before they had seen only confusion.

Another unfortunate result of the announcement of the success of the eclipse observations has been an enormous hero-worship of Albert Einstein; Pais' statement that he was canonized has now been outmatched by Miller (1996, p. 90), who states that he was deified. A result of this deification is that the greatest scorn of the scientific community is reserved for those who would try to criticize either of Einstein's theories of relativity or to suggest alternative theories, and many mainstream scientific journals reject papers critical of either theory without review. The attitude of most journals is well-described by Davies (1980) in the first sentence of his article "Why Pick on Einstein?":

> Most editors of scientific journals make special provision for coping with the huge influx of papers and letters, many bearing private addresses in California, purporting to disprove or improve Albert Einstein's monumental work on the theory of relativity.

The assessment of scientific theories, and criticisms of them, should be based on the merits of each case, rather than on the addresses from which they come.

Although Maddox (1995) describes more modern ways of measuring the deflection of radiation by a gravitational field, he appears to be willing to consider the possibility that general relativity may not be the last word; the last paragraph of his article is as follows:

> The crying need remains what it has been for the past two decades, that of marrying together general relativity and quantum mechanics. As things are, they are like chalk and cheese. It will be a great surprise if general relativity survives that marriage unchanged.

As pointed out by Earman and Glymour (1980), it was Eddington's belief that confirmation of Einstein's prediction had a beneficial effect on scientific relations between England and Germany, and it is fair to ask whether that beneficial effect was sufficient to justify the announcement of the results as being decisively in favor of Einstein's theory when they were not. It is also fair to ask, today, whether continued belief in the decisiveness of the results as announced in November 1919 is beneficial to science and scientific objectivity, or whether scientific progress would be improved by a more open acknowledgment of the inaccuracies and uncertainties in the observations. Even if it is argued that Einstein's general theory has been supported by subsequent experimental observations, that does not alter the fact that, at an extremely important scientific meeting which had enormously far-reaching consequences, the audience was misinformed by eminent scientists about the phenomenon that was the main theme of the meeting. That historical fact is not wiped out by any subsequent experimental results, whatever they may be.

Because of the euphoric veneration of Einstein and relativity in November 1919, the objectivity with which science is supposed to act has been compromised, and the search for better theories has been inhibited. Canonization, deification, and claims of personal communication from Nature, should have no place in science. If the findings of the eclipse expeditions had been announced as being inconclusive instead of decisive in 1919, general relativity would have had to compete with other possible theories, such as Gerber's, to explain certain astronomical observations, and a better theory might eventually have been found. In the author's opinion, the confident announcement of the decisive confirmation of Einstein's general theory in November 1919 was not a triumph of science, as it is often portrayed, but one of the most unfortunate events in the history of twentieth century science.

Acknowledgment

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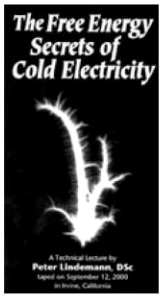

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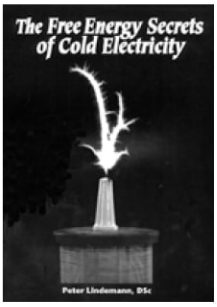

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The Einstein Myth and the Ives Papers

Eugene F. Mallove

We are delighted to reprint the 1953 paper by Dr. Herbert E. Ives from the *Journal of the Optical Society of America (JOSA)*, "Genesis of the Query 'Is There an Ether?'" This paper concisely casts the dilemma that the world of physics faced in 1953, as it still does today: Is there an aether (I prefer the old spelling), energetic or otherwise? Ives' paper was a response to P.A.M. Dirac's somewhat rhetorical query in *Nature* (1951, Vol. 168, p. 906). Ives gives perspective to the historical lineage of the great error made by Einstein, with his suggestion on how to remedy it to restore time and space to their—pun intended—time-honored roles.

Herbert Ives published some twenty-seven other articles and significant correspondence bearing on Relativity—some in *JOSA*, others in *Science, Nature, Physical Review*, and *The Philosophical Magazine*. These appeared from 1937 through 1953, long after Relativity had gained ascendancy, but during a period in which criticism of that theory was still a permissible topic in mainstream physics literature.

Unlike Einstein, who only carried out experiments in his head and was praised for doing so, Dr. Herbert Eugene Ives (1882-1953) was an esteemed, hands-on engineer with 102 patents to his name, who worked as director of electro-optical research at Bell Telephone Laboratories. We note that his technical skill on behalf of the U.S. effort in World War II received praise from President Truman. Ives also played a significant role in the development of color television. The historic photograph, reproduced here, attests to his high level of involvement; he was the project engineer on the first long-distance transmission of television in 1927.

In 1979, Vermont and Utah-based engineer Richard Hazelett and philosopher Dean Turner compiled and reprinted the Ives papers pertaining to criticism of Einsteinian relativity. The self-published, large-format, 313-page book, *The Einstein Myth and the Ives Papers: A Counter-Revolution in Physics* (ISBN-0-8159-5823-4) includes much supplementary material, and a ninety-six page "Part I" introduction by Dean Turner, "The Einstein Myth: A Presentation of Evidence for the Reality of Absolute Space and Time." Though this introduction has significant merit as a guide to the Ives' work, its frequent reference to theistic arguments, however deeply felt they may be, are unnecessary and diverting. The accomplishment is a *tour de force*, unfortunately out-of-print today. (Ed. Note: Some remaining copies are available through arrangements made by *Infinite Energy*.) Though the authors still stand by this work, according to Richard Hazelett they have found fundamental errors in the last appendix, an article by J. J. Callahan, "Euclid or Einstein: A Proof of the Parallel Theory and a Critique of Metageometry." I especially appreciated Dean Turner's appropriate remark near the end of his introduction: "Einstein was awarded the 1921 Nobel Prize. Dignity is saved by the fact that it was his early work in the theory of the photoelectric effect, rather than for his theory of relativity. However, the reader should know that Einstein rendered a trascendantly important service to the cause of freedom in 1939 and 1940, when his influence led President Roosevelt to have the atom bomb built in the United States before it was built in Nazi Germany."

It is the contention of the authors, who do not attack the experiments which supposedly back up Special and General Relativity, that these experiments strongly support the theory of Ives. They write, "Unlike other challengers of Einstein's Theory of Relativity, Ives succeeded in providing an alternate theory, which appears to answer every question that might be raised. . .Ives perfection of an alternate theory leaves intact (as it must) the still accumulating experimental results which are said to confirm the Theory of Relativity." Thus, if this Ives theoretical structure is resistant to attack, it is quite possibly among the best alternatives to paradox-laden Einsteinian Relativity. That is not to say that it is the ultimate replacement relativity theory. As I mentioned earlier in this issue, there are other contenders previously published and in development.

An extended extract from the book is a good capsule summary of the intent and flavor of this landmark book:

Einstein Myth continued on page 32

Citation to Accompany the Award of the Medal for Merit to Dr. Herbert Eugene Ives

Dr. Herbert Eugene Ives, for exceptionally meritorious conduct in the performance of outstanding services to the United States from December, 1940 to June, 1946. Dr. Ives, as Consultant for the study of black-out lighting, member of the Camouflage section and of the Division of Optics, Chief of the section of Nocturnal Logistics, and member of the Division of Physics of the National Defense Research Committee, directed important phases of the war work in the field of optics. In the scientific race against the ingenuity of the enemy, he devoted his talents to such purpose that our military equipment was superior. He directed the development of equipment which made night driving of our trucks possible under blackout conditions. He enabled ships in the presence of the enemy to signal each other without showing visible light or breaking radio silence. He developed the "Snipascope," a device by which our soldiers could detect an enemy in the dark, with which it is estimated, one-third of the Japanese casualties in the early engagements on Okinawa were caused. Dr. Ives' achievements were brilliant and his contribution to the war effort was outstanding.

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Genesis of the Query

"Is There an Ether?"

Herbert E. Ives

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Visit *JOSA* online at www.osa.org.

The recent communication by P.A.M. Dirac under the title "Is there an ether?"[1] may be taken as occasion to review the origin of the widely publicized and now almost "orthodox" view that there is no ether.

The denial of a medium for transmitting electromagnetic signals with a velocity independent of their material source was not, as is sometimes represented, a conclusion drawn from the Michelson-Morley experiment, which failed to detect relative motion between matter and ether. The existence of the ether was not questioned by Michelson; in reporting his results, he considered them as giving support to the idea of an ether entrained by the earth in its motion. In the proposals of Fitzgerald, Lorentz, and Larmor for the explanation of the null result of the Michelson-Morley experiment there is no suggestion of the nonexistence of the ether. The contractions of dimensions and clock rates associated with their names were suggested behaviors of matter in motion *with respect to the ether,* in conformity with which the Michelson-Morley experiment would not reveal the motion.

Neither was the denial of the ether a result of the formulation of the principle of relativity. This principle, which we owe to Poincaré (1904), denied the possibility of an observer "carried along in a uniform motion of translation" of discovering (*i.e.*, by measurement) such a motion. It is abundantly clear from Poincaré's writings that the "motion" in question was motion *with respect to the ether.* Poincaré identified the principle of relativity with the mechanics of Lorentz, as embodied in the Lorentz transformations. These transformations, put in their now familiar symmetrical form by Poincaré, were built up by Lorentz from the contractions of dimensions and clock frequencies invented by him to explain the behavior of matter *with respect to the ether.* The suggestion that the ether is nonexistent is not to be found in either Poincaré's or Lorentz's discussions of the principle of relativity; in fact Lorentz explicitly affirmed his adherence to the ether.

The idea that there is no ether appears apparently for the first time in the paper by Einstein in 1905, "On the Electrodynamics of Moving Bodies," in the statement in his preliminary remarks: "The introduction of a 'luminiferous ether' will prove to be superfluous, inasmuch as the view to be here developed will not require 'an absolute stationary space' provided with special properties." It is this statement, and its acceptance as proved by the reasoning of Einstein's paper, which is to be credited with "the abolition of the ether." It is consequently pertinent to investigate the justification for Einstein's claim and the validity of his supporting arguments. Did he in fact furnish a sound and adequate substitute for the ether of earlier theory?

Divested of his questionably pertinent discussion of simultaneity and his arbitrary *definition* of the time for a light signal to travel out as always equal to the time back, the significant feature of Einstein's development is as follows: He prescribes that the "velocity" of a light signal traveling in one direction along two different parallel paths in uniform relative motion shall be the same, and equal to *c*, which "in agreement with experience" is "a universal constant. . .the velocity of light in empty space." From this proposition he derives the Lorentz-Poincaré transformations, and with them the Fitzgerald-Lorentz-Larmor contractions of lengths and clock frequencies, these latter entirely as phenomena of relative motion of matter.

Now the proposition that any independent signal velocity shall be the same with respect to all other independent velocities is, in terms of Newtonian "absolute" space and time, a sheer paradox. It is *not*, as sometimes stated, a requirement of the principle of relativity. All that the principle of relativity requires is that any specific set of observations or measurements which we may make on the transit of light signals with respect to one body shall yield the *same* result on any other body in uniform motion with respect to the first *when made by instruments partaking of the motion*. It is therefore of the first importance to be clear as to what observations and measurements we make when we attempt to determine what, in a Newtonian system, would be "the velocity of light."

Two different measurements are to be distinguished. The first is of signals sent *out and back* to one clock at the origin, which is unmoved, except in so far as it partakes of the motion of the system. The "velocity" of light is then the quotient of the measured distance traversed by the signal by the time indicated by this single clock. This is the "experience" quoted by Einstein. In accordance with the principle of relativity this measurement always yields the same result; in accordance with the Michelson-Morley experiment this result is always the constant *c*. The second measurement is of a signal sent in *one direction* from a clock at the origin to a second clock at the end of the path. The second clock must be set as to epoch with respect to the first by some physical manipulation. The only such manipulation which is observable and measurable, not dependent on arbitrary definition, is to move a clock from one end of the path to the other at an observable rate measured by the distance passed over and the time as measured by this setting clock itself. The resulting epoch will of necessity be some function of this self-observed clock velocity, hence the expression describing the epoch will contain terms involving the setting clock

The idea that there is no ether appears apparently for the first time in the paper by Einstein in 1905, "On the Electrodynamics of Moving Bodies. . ." The reasoning of Einstein's paper. . . is to be credited with "the abolition of the ether."

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velocity. By the principle of relativity the "velocity," more properly the "rod-two-clock quotient" of light so measured will be the *same* on all platforms in uniform relative motion, for the same choice of setting clock velocity, but it will not be the same as the one-way "velocity" which contains no clock velocity terms.

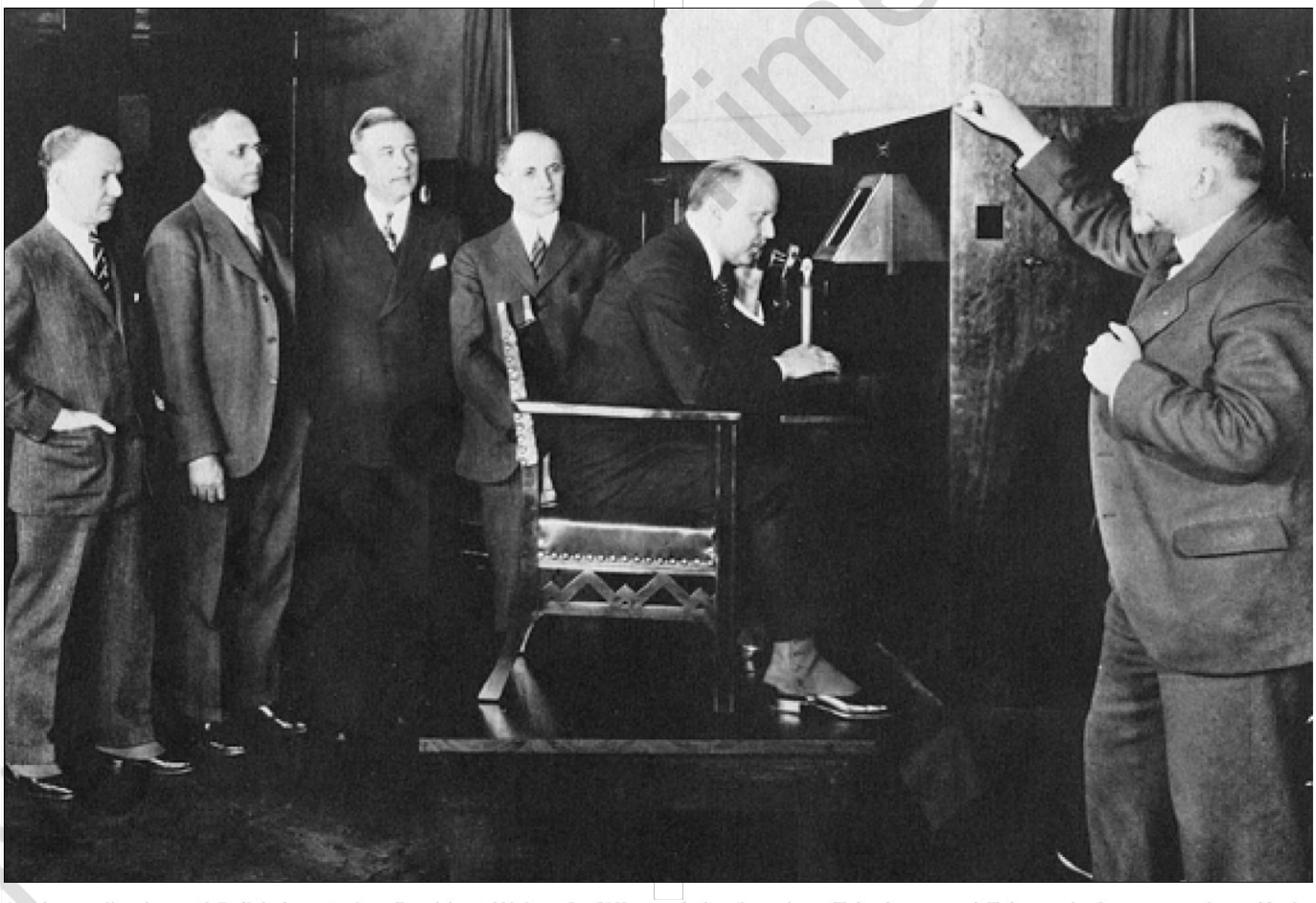
The consequence of the loose identification of two different "sames" is an internal contradiction in Einstein's "view." In his derivation of the Lorentz-Poincaré transformations he postulates the one-way velocity of light to be c. From the contractions of length and clock rate with motion contained in these transformations it is possible to determine the epoch of a moved clock. When this is done and time is measured by the moved clock, the "velocity" of a one-way signal turns out to be a function involving the moved clock velocity, that is, it is *not c,* contradicting the initial postulate. This has been recognized in the Special Theory of Relativity to the extent that the use of moved clocks for establishing distant epochs is prohibited (or they are to be moved "infinitely slowly," which means the measurement would never be made!). Instead, distant clock epochs are prescribed to be made by light signals *assigned* the velocity c, by which indeed the resulting measured value is c, but this is a rigmarole, not a legitimate measuring procedure. Einstein's "constancy of the velocity of light" is not a competent substitute for the ether.

This inconsistency of the Einstein procedure is to be contrasted with the treatment of the problem by considering the velocity of light as a constant c with respect to the ether, and applying to our measuring instruments the contractions of length and clock frequency as occurring on motion with respect to the ether. The procedure of setting distant clock epochs by moved clocks then gives the "velocity" of a one-way signal as

$$\frac{c}{1-\frac{c}{q}\left[\left(1+\frac{q^2}{c^2}\right)^{\frac{1}{2}}-1\right]},$$

Where q is the self-measured velocity of the setting clock. The same reasoning results in "transformations" of the Lorentz-Poincaré type containing terms in q, which approximate to the Lorentz-Poincaré formulation when q is small. The principle of relativity is conformed to, there is no paradox, no internal contradiction, no prohibition of the use of clocks, no resort to "definition" unsupportable by measurement.[2]

Further ground for rejecting the claim that Einstein's "view" renders the ether superfluous is furnished by the consideration that his scheme (even if it were valid) applies only to uniform rectilinear motion; rotational motion is excluded, yet optical signals are transmitted in such systems, and with results which point clearly and unequivocally to their trans-



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In the auditorium of Bell Laboratories, President Walter S. Giffore of the American Telephone and Telegraph Company, views Herbert Hoover, then Secretary of Commerce, in Washington, D.C., as they converse during the first long-distance demonstration of television, April 7, 1927. Radio transmission from Whippany, New Jersey, was demonstrated on the same occasion. With Gifford as he talks and sees over two hundred miles of telephone lines are members of Bell Telephone Laboratories. From left to right are E.P. Cliffore, Vice President; H.D. Arnold, Director of Research; E.B. Craft, Executive Vice President; F.B. Jewett, President; and Dr. Herbert E. Ives, technical director of electro-optical research. For details see Bell Laboratories Record, Volume 4, No. 3, May 1927; also *New York Times,* April 8, 1927, p. 1.

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mission in an independent medium or coordinate system. If light signals are sent out simultaneously in the "fore" and "aft" directions from a source moving in a circular path, and the two signals are brought back to the source by a series of reflections, they do not arrive simultaneously; the source has moved forward to meet one signal and has moved away from the other. This is the situation in the Michelson-Gale and Sagnac experiments, which yield positive first-order results exactly in conformity with the idea that the light signals travel in a fixed ether. The contractions of length and clock frequencies which account for nul effects in uniform motion of translation, being of the second order, do not materially alter the rotational effects. The optical phenomena in both uniformly and rotationally moving systems are completely explainable by a fixed ether and the Fitzgerald-Lorentz-Larmor contractions.[3]

This survey of the background of the query "Is there an Ether?" shows that the grounds for "abolishing" the ether were mistaken, and consequently Dirac's contribution would more properly have been entitled "Properties of the Ether Suggested by Recent Speculations."

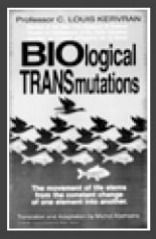
1. Dirac, P.A.M. 1951. *Nature,* 168, 906.
2. The above argument will be found in greater detail, with full references, in Ives, H.E. 1951. *Proc. Am. Phil. Soc.*, 95, No. 2, 125, April.
3. See Ives, H.E. 1952. *Proc. Roy. Dublin Soc.*, 26, 9, May 1.

Einstein Myth continued from page 29

When a dissenter enters the academic citadel of physics, he obviously treads on holy ground. No one is to trifle with the mystique by which the physics profession and its hangers-on maintain their high prestige. For seventy-two years humanity has been browbeaten by an incomparably brazen bit of pseudo-science, because its perpetrators have defended it by using mathematics which, though valid in itself, is not applied to objective facts that are analyzed logically in the real world. Recondite kinds of higher mathematics have been falsely used to create an awesome, esoteric language whereby the initiated elite have set themselves apart from the world and have labeled all dissenters as quacks.

But physicists: Where is the theory of relativity by now? Gone is Einstein's self-consistency, and gone the claim of relativity to exclusive explanatory power over the relevant experimental data. Gone is the motivation for relativistic interpretation of Minkowski's space-time geometry, and gone is any sensible motivation for using non-inertial reference frames.

To be contrasted with this cerebral disaster is the Absolute Space and Time Theory of Ives which has full predictive power over all the known facts, is self-consistent, and is altogether derivable from classical physics plus Ives' restricted principle of equivalence. The results appear to be the greatest combination of coherence and explanatory power achieved in physics to the present time. For this achievement, Henri Poincaré deserves more credit than he has been accorded, in view of his numerous constructive insights. ❑❑❑



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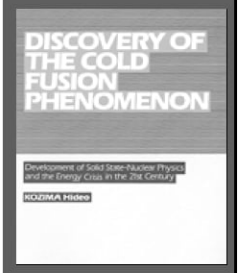

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The Domain of Special Relativity

Robert S. Neiswander*

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A unicorn may be defined as a horse plus a horn. That horses abound, however, does not in itself establish the existence of unicorns. A Special Relativistic system may be defined as one that exhibits relativistic timekeeping: motion-sensitive time dilation plus position-sensitive time offset. That time dilation effects abound, however, does not in itself establish the existence of Special Relativistic systems.

Introductory Remarks

For nearly a century, the Special Relativity Theory has been a cornerstone of modern physics, and has, at least so its admirers claim, survived all challenges. ". . .It now looks very unlikely that Einstein's ideas can be falsified in any superficial sense. Experiment after experiment bears them out. . . The successor of Newton and Einstein must instead look for extreme conditions which prove the equations inadequate and probe into the half-hidden electronics of the universe.[1]

But not all wise men are in accord. From its very inception, Einstein's theory of Special Relativity has been questioned by competent physicists ranging from Hendrick Antoon Lorentz and Herbert Ives to more recent criticisms by Petr Beckmann and Thomas Phipps, Jr. Is Special Relativity a frail abstraction built upon *"hunches"* and *"guesses,"* a ". . . theory is so incredibly sharply tuned that it can be produced only through the particular set of idealizations Einstein employed—notably, the assumption of mathematically rigorously unaccelerated motions?"[2] Or, is Special Relativity truly monolithic and unassailable?

During the past nine decades, we have learned a great deal about space and time, so perhaps a reassessment of the true role the theory of Special Relativity plays in our universe is appropriate. To be eligible for the domain of Special Relativity, a system must keep time in accordance with the law as defined by Einstein's transformation. If it disobeys the law, the system obviously must be banned from the domain.

But how do we know whether a system's clocks perform legitimately or not? This is our mission. After briefly reviewing the basis for relativistic timekeeping, we play "What if." What if Special Relativity were enforced in a typical real environment? The consequences lead to a single, succinct exclusionary rule. This rule immediately defines the domain of Special Relativity and it establishes the qualifications for all tests acclaimed to "prove" the existence of Special Relativistic systems.

1. Relativistic Time

The huge leap taken in Einstein's notion of relativity was introducing a light ruler that converted time to distance ($x_0 = ct$) and enfolding this distance into a four dimensional geometry. This was not merely an artifice used to simplify equations, it was and is taken by modern physics to represent the intrinsic nature of the universe. By definition, the speed of light must be isotropically constant within any localized region. Should, for instance, some new experiment establish that the speed of light were $c + \delta$ in one direction and $c - \delta$ in the opposing direction, then even if δ were minuscule—say, one part in 10^{10}—the vast relativistic edifice would be put in jeopardy.

So by modern view, time, t, can be treated just like ordinary spatial dimensions, x, y, and z—sort of. The problem is that unlike x whose square is a positive number, the square of t is a negative number. We must either take time to be imaginary or carry along a sign adjustment factor applied whenever time is squared. The easiest way to avoid confusion is to always extract time as a separate component: a four-dimensional vector $\mathbf{r}$ is split into $\mathbf{r}$ = (time, three-space). For example, a clock stationary at the origin of a coordinate frame marks off a unit time $\mathbf{e} = (1,\vec{0})$, and a clock a meter away from the origin has $\mathbf{e} = (1,\vec{1})$.

Now, let's simplify the notation. For *time* we use ct, same time but now in meters rather than seconds. Velocity, say, in the x direction becomes $x / ct = \upsilon / c = \beta$, where υ is ordinary velocity in meters per second, and β is dimensionless. Moreover, we assume that coordinate frame K' moves in the x direction at velocity β with respect to a stationary frame K.

With these preliminaries out of the way, the rest is easy.

Motion-Sensitive Time

We start with a unit time interval marked off by a clock at the origin of the stationary frame, K. Its vector is $\mathbf{e} = (1, \vec{0})$. In the moving frame this vector splits into a time component h during which the frame moves a spatial distance βh. (Geometers call h the *height* of the vector.) The base vector must remain intact regardless of how it is viewed or split, so the split at the moving frame must be $\mathbf{e} = (h, \vec{\beta h})$. It is a right triangle, if you will, with a hypotenuse 1 and sides h and βh. The new time axis has rotated an angle whose tangent is β. Pythagoras says the triangle sides are related by, $(1)^2 = (h)^2 + (\beta h)^2$. Remembering that time squared is negative, we get,

$$-1 = -h^2 + h^2\beta^2; \text{ or } h = (1 - \beta^2)^{-1/2} = \gamma \qquad (1)$$

Thus, the time intervals in the two systems relate by $\Delta(ct) = \gamma\Delta(ct')$. An interval marked off by the moving clock is "dilated" by the γ-factor when viewed from the stationary frame. Note that this factor is created specifically by splitting the stationary pure time vector into space-time components at the moving frame. What is *time* in one frame becomes *time* + *space* in another frame.

Position-Sensitive Term

The x-axis of stationary system K is perpendicular to its time axis, and the x'-axis of system K' is perpendicular to its time axis. Therefore, the x'-axis is also rotated by $\tan^{-1}\beta$, and the unit $\mathbf{x}$-vector splits into, $\mathbf{e_x} = (\gamma\beta, \gamma)$. If the displacement is $\Delta x'$, then the time component is $\Delta(ct) = \gamma\beta\Delta x'$. That is, due to the interchange of time and space, a position off-

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set $\Delta x'$ at the moving frame produces a time offset $\Delta(ct)$ as viewed at the stationary frame. Summarizing, the composite time observed at K is,

$$ct = \gamma ct' + \gamma\beta x' \qquad (2)$$

which is, of course, the Einstein-Lorentz transform for time.

Both the motion sensitive dilation term and the position sensitive offset are direct consequences of splitting time in one frame to *time + space* in the other frame. They are inseparable. If the time dilation effect exists then the time offset must also exist; this is a fundamental feature of all systems that lie within the domain of Special Relativity.

Alternative Theories

A hundred years ago Newtonian mechanics was entrenched and Maxwell's explanation of electromagnetic phenomena had emerged. Despite a number of theories involving various kinds of luminiferous aether, mechanics and electromagnetics remained largely irreconcilable until Einstein proposed his four-dimensional geometry theory. The subsequent contest between the classic theories and Einstein's relativity was short lived, for indeed his theory alone provided the sought for answers. By dichotomy, any effect that deviated from Newtonian physics was *relativistic*, and was taken as unarguable proof that Einstein's four-dimensional universe prevailed.

But the challenges to relativity are, today, much different from those of the classicists. For example, one can accept the speed of light as the limiting velocity for any particle, but take the particle or system velocity to be with respect to a specific rather than a relative frame of reference. Several modern theories postulate that velocities at and near the Earth are with respect to a non-rotating geocentric frame. This is not a relativistic option, it is a demand. And consequently the γ-factor and its effect upon time dilation are independent of the observer. The clock in the GPS satellite reads the same time irrespective of the motion or location of the observer.

And a few of these theories, such as those of Petr Beckmann[3] and Ron Hatch,[4] provide much simpler, more direct explanations of the behavior of GPS satellite clocks than that offered by relativity. The γ-factor is acknowledged, but there is no corresponding time offset with position offset, no commingling space and time.

Therefore, unlike the interpretations a hundred years ago, the detection of time dilation or of mass increase with velocity is no longer, in itself, unique evidence that relativity prevails. To establish with conviction that the system's behavior is properly relativistic, the observations must demonstrate that the inseparable effects, time dilation and time offset, both occur. If time and space are truly four-dimensional, then one effect cannot exist without the other.

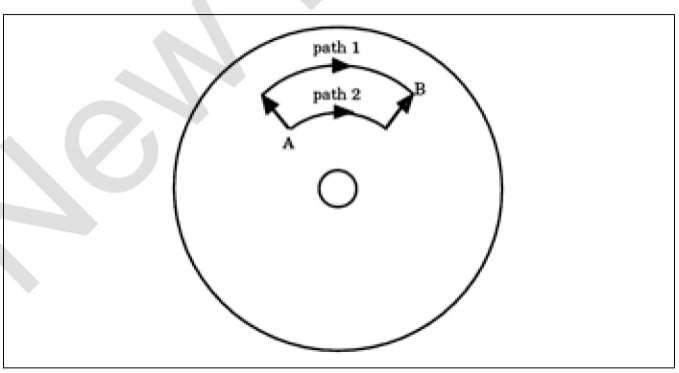

Figure 1. Two different paths from point A to point B illustrating the path dependency of the resultant time offset.

2. The Elusive Time Offset

Rotating systems dominate the palpable Universe—the Earth spins as it orbits the Sun that orbits within the Milky Way and so on. Therefore, to parallel reality, we choose for the model geometry a gently rotating reference as viewed from a non-rotating frame. Moreover, we assume that the gravity potentials are known and the proper timekeeping corrections invoked for all time measurements. Thus, the problem reduces to relativistic kinematics. With this model, we develop a general rule for characterizing rotating systems, and then inspect the ramifications.

The Relativistic System in Rotation

Let's first consider two satellites moving along an identical circular orbit and spaced a kilometer apart. We know from a trove of GPS satellite experience that the timekeeping rate of clocks aboard the satellites is slowed by the time dilation factor $\gamma_s = (1 - \upsilon_s^2 / c^2)^{-1/2}$, where υ_s is the peripheral velocity of the satellite with respect to a non-rotating, geocentric frame. A master clock at a ground station is corrected for the Earth's spin so that it reads out time as if it were at the origin of the geocentric reference.

Now we have a choice. We can synchronize the two satellite clocks by a cross-link and then observe their times from the ground station; or, we can synchronize the satellite clocks from the ground station (much in manner of GPS satellite timekeeping), and note the time offsets with respect to the moving system. Here, we follow the second option. By ground links, the satellite clocks are set to zero (at $ct = 0$). If the satellite pair were properly inertial and abided by the laws of Special Relativity, then satellite number 1 would observe a time offset in number 2 lead satellite's clock in accordance with the Einstein-Lorentz time transform,

$$ct = \gamma_s ct' + \gamma_s\beta_s x' = 0; \text{ or } ct' = -\beta_s x' = -\upsilon_s x' / c$$

Suppose that the satellites are at GPS altitude, their peripheral velocity is 3870 meters per second, and the resultant time offset is -43 picoseconds. If we introduce a third satellite spaced a kilometer from the second, then its clock would register -43 picosecond offset compared to number 2's clock and -86 picoseconds compared to number 1's clock. Now, adding satellites a kilometer apart until a diametrically opposite orbital point is reached, we produce an accumulated time offset of almost -3.6 microseconds.

If we went through the same exercise using trailing rather the leading satellites, around the orbit in the other direction, the velocity reverses sign and the accumulated time offset at the opposed orbital point is now +3.6 microseconds. *The time offset depends upon the accumulation path taken between the two points.*[5]

To get a more general picture of this effect, consider two arbitrarily located points, point A at radius R_1 and point B at R_2 from the center of a coordinate system rotating at Ω radians per second (see Figure 1). The angular spacing between the points is θ. As with our satellite example, the clocks at the two points have been zero set by a master non-rotating clock at the hub. Keeping in mind that radial displacements

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have no affect upon the time offset, we find the time offset for two paths from point A to point B.

Path 1: The path proceeds along radius R_1 until it reaches R_2 and then along the arc with R_2 constant until it reaches the second point. The accumulated time offset is:

$$t' = x'\upsilon/c^2 = (\theta R_2)(\Omega R_2) \,/\, c^2 = \theta R_2^2\ (\Omega/c^2)$$

Path 2: This path maintains a constant radius, R_1 until it meets the radius line of the second point, and thence along the radius to the end point. Consequently the accumulated time offset is:

$$t' = x'\upsilon/c^2 = (\theta R_1)(\Omega R_1)/c^2 = \theta R_1^2 = (\Omega/c^2)$$

Path difference: $\Delta t' = (\theta R_1^2 - \theta R_2^2)(\Omega/c^2)$ (3)

This difference in time offset is proportional to the area bounded by the two paths. Since the difference is not zero, we can set out as a general rule that:

> *The relativistic time offset between any two points within a rotating system is dependent upon the integration path taken between the two points.*

Before we go further, we should touch base with reality. What is a *rotating system*? In the same spirit that Einstein and his mentor Ernst Mach defined a non-rotating *inertial system,* we define a rotating system to be one that exhibits centrifugal forces or other signs of rotation in the presence of the massive universe. The definition is flawed in the sense that it does not explain *why* the unique non-rotating state exists.

The Consequences

So far, we have avoided controversy. Now, however, we depart from this safe house by setting out the consequences of the path dependency rule.

First, let's consider a typical GPS satellite wheel. As noted earlier, we can either set all satellite clocks to proper time in the rotating system, or we can set the four satellite clocks to zero, synchronized to a central clock at the origin of a non-rotating geo-centric coordinate frame. For the latter case, if the wheel is appropriately inertial, relativistic time offsets will be observed satellite to satellite. For the route from satellite number 1 to number 3 via satellite number 2, a negative time offset is accrued. A direct route across the orbit diameter from number 1 to number 3 produces no time offset. And the route to number 3 via satellite number 4 produces a positive time offset. Three different times at satellite number 3 clock can be obtained by simple route selection

Now we add a return trip from number 3 to number 1. Each of the three times produces multiple time offsets back at satellite number 1. So, with successive round trips and differing routes, we can create an unending series of time offsets at all four of the GPS satellite clocks. Since this result is in overt conflict with reality, it leads to the following postulate,

> *Within any real system in motion, the time offset between any two points must be independent of the integration path taken between the two points.*

That is, the system clocks must read out single-valued times. Combining this postulate with our general rule for time offsets, leads to the conclusion: *All rotating systems must be excluded from the domain of Special Relativity.* Sure. Everyone agrees, but usually assumes that, like the speed limit on a freeway, the law is "flexible"; if you disobey it just a wee bit—no problem.

What if we reduce the extent of the system and coincidentally slow its rotation rate? In our initial system the two little satellites find their spacing of a kilometer contracted to a hundred meters and then one meter. Meanwhile the orbital rotation reduces to once around every month and then once a year. Unfortunately, the path length discrepancy doesn't go away, it simply scales down. Time and space still don't mix properly. If one argues that the reduced system is acceptable because the offset non-effect is, today, too small to be confirmed, then tomorrow, with better instruments, no offset is detected and the system is expelled from the Special group. You see the problem. The system cannot be even a tiny bit pregnant and remain a virgin.

If this judgment is correct, then no physical experiment can be qualified for Special Relativistic status. In the real rotating universe, on any scale, time and space do mix in accordance with the restricted rules.

A harsh judgment indeed! What about the stacks of evidence that "prove" Special Relativity is alive and with us?

3. The Evidence

The review here by no means attempts to be comprehensive. It simply illustrates by sampling what some of the prominent tests made over the past century actually prove.

Timekeeping

A wide variety of data ranging from measurements of the time dilation of μ-mesons[6] to that of airborne[7] and satellite borne clock rates all confirm the existence of the γ-factor slowing the moving timepieces. This shows that Special Relativity and a variety of modern theories are not incorrect, but nothing more.

So let's look inside the factor. What is the velocity, the β, that determines γ? For the meson experiment, the reference velocity was with respect to the laboratory. If the spin velocity of the Earth happened to be added in, who's to know? On the other hand, Hafele and Keating's airborne clocks clearly demonstrated that the velocity at each clock was referenced to a non-rotating geocentric frame. Exactly the same results have come from all satellite measurements, including the extensive GPS operations. While this fact can be contrived to fit within Special Relativity, the much more forthright and far simpler explanation is given by theories such as those mentioned in the first section.

Increase in Mass with Energy

Early on, tests of high speed electrons and whatnot confirmed that their inertial masses increased with an increase in kinetic energy. The photon, the embodiment of pure kinetic energy, has an inertial mass. This again merely demonstrates that the γ-factor exists and that several theories are not disproven.

Relativistic Doppler Effect

Confirmation of the second order relativistic Doppler effect[8] has been acclaimed as another triumphal victory for Special Relativity. The relativistic influence upon the Doppler effect arises solely from time dilation at a moving source. So again, this genre of tests verifies the existence of the γ-factor, not the validity of Special Relativity.

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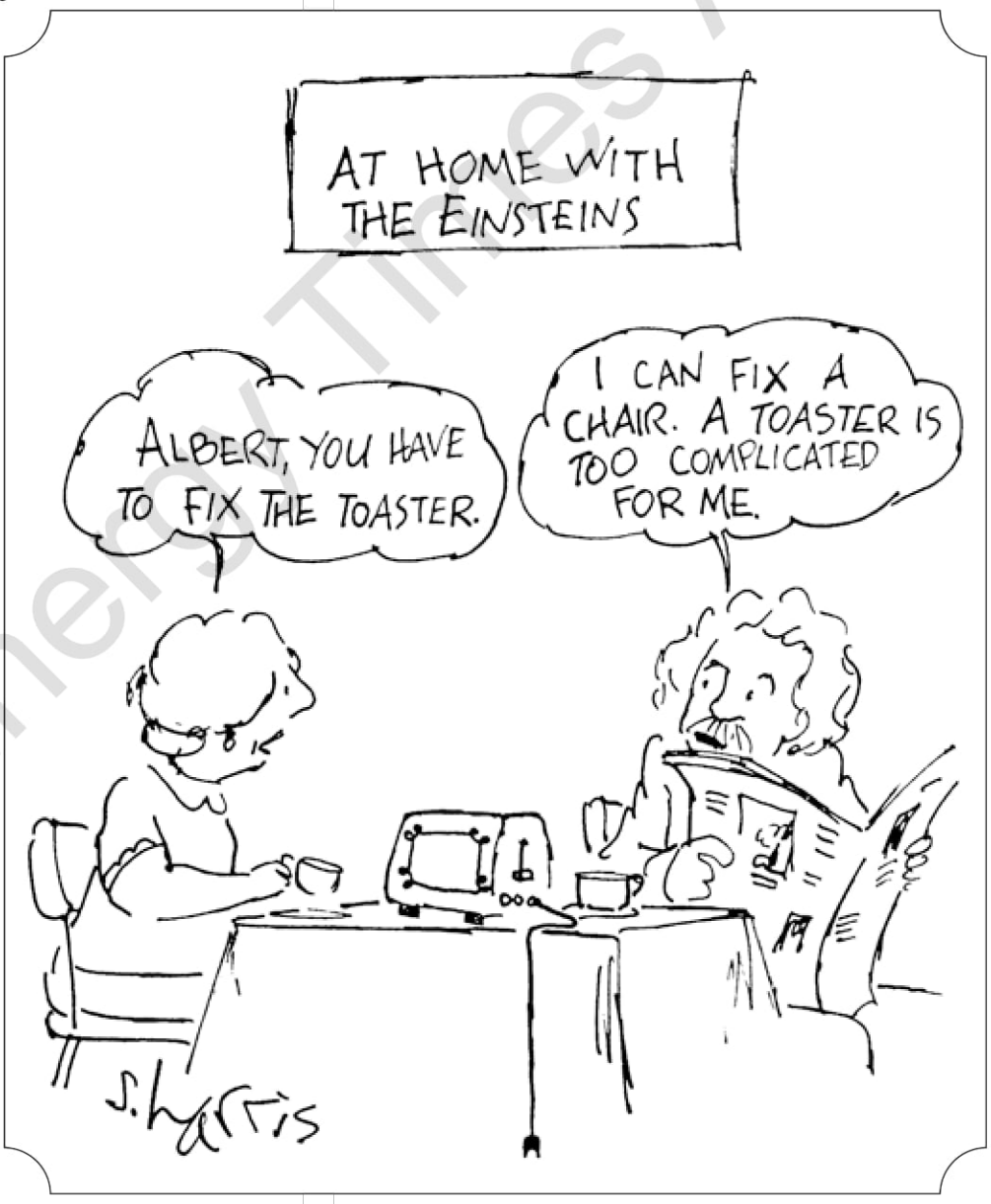
Speed of Light

Of all of the "Michelson-Morley" type tests made in the past century, only the rotating table experiment conducted by Brillet and Hall[9] had a sufficient accuracy to detect a light speed anisotropy due to the Earth's spin. And though the results showed a steady state "offset" of about 150 meters per second, this was dismissed as due to some unknown factor. A number of experiments have been made with fixed or reversible setups, but these could only detect the sidereal variation in light speed, that is, the contribution from a fixed aether attached to the Sun or to the cosmic microwave center of the universe. One test, run by Champeney *et al.*[10] used a rotating table and could measure steady state velocities. But their interpretation of the results was most curious (see Howard Hayden's critique, Reference 11); the transit time of light was considered but the slowing of the Mössbauer element "clocks" with motion was ignored. If this well-established effect is included, the anisotropic model gives the same null result as the isotropic model.

The conclusion is that proof of the behavior of light is claimed by all sides. But there has not as yet been a definitive, undebatable test.

4. Recapitulation

We have shown that time and space do not display acceptable Special Reativistic four-dimensional behavior in any rotating system, and have then argued that this behavior disqualifies all real systems for Special Relativistic status. To support this view we note that none of the experiments performed in the past century have revealed the time-space mix predicted by Special Relativity. Although many tests have shown that the γ-factor is real, none have demonstrated that its inseparable companion, the time offset, exists. The experiments merely confirm that several alternative theories and Special Relativity are not disproved. The conflict between relativity and modern competing theories has in no sense been laid to rest.



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Axis Calibration: The Thing Einstein Forgot

Thomas E. Phipps, Jr.*

Abstract

We suggest that in 1905 Einstein was careless about the calibration of his spatial coordinate axes, and that this oversight has never since been corrected by relativists or logicians. In special relativity we find that longitudinal acceleration of a measuring rod "deconstructs" it, with the consequence that there exists no reference frame in which it is at all times measured as stress-free (*i.e.*, as qualified to serve as a metric standard). An alternative theory is outlined that corrects this by restoring classical rigidity as a permissible idealization. This theory necessarily alters both electromagnetism and kinematics in complementary ways.

Introduction

Toward the end of his life, Einstein remarked that he wouldn't want to be starting over again. He died in 1955, at which time he didn't know the half of it. Had he lived another fifty years, he wouldn't have wanted to start to be starting over. For by that time the character of physics had changed: The Einstein doctrines had been set in concrete and the kind of heretical departure from the status quo that his special relativity theory (SRT) originally represented had become "dissidence"—despised, ridiculed,[1] and banned from the literature[2] by all properly-indoctrinated, right-thinking physicists. In that short but fateful interval of time, Albert Einstein had become the new Claudius Ptolemy, and the little world of professional physics had voluntarily condemned itself to a thousand years of trimming-down the great world to fit into a bed of 1905 philosophical truth.

Einstein surely, and Ptolemy probably, deserved better of his minions. Rather than emulating his thought, they should perhaps have emulated his thought processes. Instead, they were hell-bent to do the easy thing—to make physics into a religion built on incontestable truths. And SRT, together with Maxwell's equations, looked like the rock on which to found such a church. Even Dirac, the cat who walked by himself, accepted that complementary pair of truths—although he warned that people who did the same for quantum mechanics were in for a shock someday. (He said, "One shouldn't build up one's whole philosophy as though this present quantum mechanics were the last word. If one does that, one is on very uncertain ground and one will in some future time have to change one's standpoint entirely."[3])

Like lesser thinkers of all times, Einstein was limited by what he knew, and more importantly by what he didn't know. He knew Maxwell's electromagnetic theory, but (rather surprisingly) not Hertz's.[4] As a result, his ignorance has propagated itself into the present era; so that hardly any physicist now alive is aware that Heinrich Hertz (a formidable theorist in his own right, although today credited only as the experimentalist whose work apotheosized Maxwell) published a generalized formal theory of electromagnetism[4] that corrected the failure of Maxwell's equations to be invariant under first-order (Galilean or "inertial") coordinate transformations. To this day, physicists will tell you that it is impossible to write down field equations for electromagnetism that are genuinely *invariant* at first order. That's because they don't read the literature. They rely on scholars and textbook writers to do that for them. . .which is to say, they lean on a weak reed—in fact, on serried generations of reeds leaning on reeds.

Hertz did the "impossible" (although it requires updating of his notation, etc., to see it) by producing an *invariant covering theory* of Maxwell's electromagnetism—that is, a broader theory that does everything the lesser theory does and other things besides. He accomplished this by the elementary trick (so simple it never occurred to him to make a big thing of it) of replacing Maxwell's first-order noninvariant partial time derivative operator $\partial/\partial t$, wherever it appeared, by the *invariant* total time derivative operator. In its simplest form this operator is

$$\frac{d}{dt} = \frac{\partial}{\partial t} + \mathbf{v}_d \cdot \nabla \ ,$$

$\mathbf{v}_d$ being a new velocity-dimensioned parameter foreign to Maxwell's equations. Hertz mistakenly interpreted this parameter as ether velocity, since ether was on everyone's mind toward the end of the nineteenth century; but it seems more apt (and more in harmony with operational definability) to interpret $\mathbf{v}_d$ as *field detector velocity* with respect to the observer's inertial system. This corrects a genuine oddity of Maxwell's equations—that they parametrize field source motions but not field sink (detector or absorber) motions.

Significantly, the Hertzian formulation fits with "Faraday's law," written[5] as

$$\oint_C \mathbf{E}' \cdot d\mathbf{l} = -k \frac{d}{dt} \int_S \mathbf{B} \cdot \mathbf{n} \, da \ ,$$

where the total time derivative appears explicitly, as it must in order to allow for arbitrary shape changes of the circuit C. There is of course no legitimate way (true to Faraday's observations and consistent with the extra parametrization involved) that this invariant total time derivative can be made equivalent to a noninvariant partial time derivative, as required to yield Maxwell's equations. Hence "fast talk"[5] is employed to do the job. (There's more of that in "accepted" physics than you might think: voodoo science[1] in the classroom?) When $\mathbf{v}_d = 0$ the detector comes to rest at the observer's field point and Maxwell's special case, and his equations, are recovered via

$$d/dt \rightarrow \partial/\partial t \ .$$

By making this simple formal change in Maxwell's equations, plus concomitant modifications of the source terms, Galilean invariance of the vacuum field equations is attained, as needed to effectuate the relativity principle *at first order* (an order not addressed by SRT, which is founded on the *second-order* Lorentz transformation!). The proof of Galilean invariance of Hertz's equations has been given in numerous places, including this magazine.[6,7,8] The Hertzian field quantities have different operational definitions from their Maxwellian counterparts,[7] and different attributes. For instance, the one-

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way Hertzian phase velocity of light is constant only if the field detector is at rest at the field point (the usual case in laboratory practice). Once one gets physical relativity established at first order via true mathematical invariance, one can go on from there to establish higher-order invariance[7] and need never look back (*e.g.*, at covariant expedients and the Lorentz transformation, which never arise in the first place except through skipping over first-order physics).

So, the Maxwellian rock is a bit mushy. What about the SRT rock? I should characterize that as quicksand. To begin with, it leans on Maxwell to justify "universal covariance," surely in any mathematician's view a contrived (though clever) and second-best substitute for genuine invariance. Beyond that, SRT projects the image of a purely logical construction of the physical world from first principles. As such, although perhaps not directly a contradiction in terms, it certainly courts nonsense. For an output of physics without an input of the same is magic: something for nothing. It is this aspect of Einstein's work—this "dream" of doing it all out of the human head—that harks back to Greek science and has had the most pervasive and pernicious effect on modern physics. Suddenly, empiricism is for second-raters. Observe the result: To justify the way they "put together" the world, relativists focus on a plethora of single-particle experiments that tell them what they want to hear. Despite their addiction to pontifications about the way the world is *structured*, they never lobby for experiments employing actual extended structures. Such would be too difficult. . .it is cheaper and easier to theorize, to "accept" theory through consensus, and to build more theory on accepted theory—until the Ptolemaic millennium passes and the whole edifice of "truth" collapses with a whimper.

Axis Calibration: The Lorentz Contraction

Is there any substantive ground for suspecting that the SRT description of extended structures may be fundamentally faulty? The theory conveys the impression of being almost unique among human intellectual efforts in the degree of relentlessness of its logic. Therefore it must be built to withstand logical criticism. My criticism, which I hope to clarify here, is that spatial axis calibration has been neglected. No doubt this seems pedantic to the reader, but the theory itself is the ultimate essence of pedantry, so we must be pedantic in order to grapple with it on its own turf. Obviously, if one is going to propose a revolutionary, nonintuitive coordinate transformation as the basis for all future physics (universal covariance), one needs to be able to tell some story about how numbers are assigned to "coordinates" in different inertial frames of reference.

Let us consider spatial coordinates. The purely "kinematic" way to calibrate spatial axes is to have a master length standard at rest in system S and to transfer it (or a replica) to a state of rest in S' by some protocol of force application that leaves it at all times *stress-free*. In that way we can be sure that the transferred standard remains continually a true replica of the master. (Stress is not part of kinematics, but we shall overlook this, as well as the fact that SRT itself gratuitously generates oxymoronic "kinematic stresses" such as those of Herglotz or Dewan-Beran.[8]) It is essential to note that the "stress-free" condition of a structure means entirely different things in SRT and in classical physics. In SRT it means "Born rigid," which by definition is the same as continually Lorentz contracted (*i.e.*, strained-but-not-stressed by relative motion). In classical physics it means classically rigid, which is the same as length-invariant (stress-free = strain-free). In what follows this crucial distinction must be kept in mind.

Since axis calibration involves acceleration and SRT by its nature addresses unaccelerated motions, there might seem to be a problem. Remember, at the time we set about calibrating our axes we do not yet have them calibrated—that's why we are doing it—so we cannot logically avail ourselves of the Lorentz transformation. To claim that coordinate values can be quantified without performing prior operations to calibrate axes would be as logically circular as supposing that observers in S and S' both know what a "meter" is because they look up the word in the same dictionary. How did Einstein deal with this? In 1905 (and ever after) he finessed it.[9] In one place in his 1905 paper he said, "We now imagine the axis of the rod lying along the axis of x of the stationary system of coordinates, and that a uniform motion of parallel translation with velocity v along the axis of x in the direction of increasing x is then imparted to the rod." In another place he said, "Now to the origin of one of the systems *(k)* let a constant velocity v be imparted in the direction of the increasing x of the other stationary system *(K)*, and let this velocity be communicated to the axes of the co-ordinates, the relevant measuring-rod, and the clocks." That is, motion is "imparted" either to the rod or to the frame in which it stays at rest. (Remember, at that time Einstein still believed in "rigid rods" in the classical sense. In his final years he was reported by Shankland as still interested in the subject of rigidity. And well he might have been. Maybe the old fox was not tranquilized by fast talk of von Laue and others about the *need* for causal "communication" among the parts of a structure. If that "need" ruled out classical rigidity, it equally ruled out Born rigidity!)

In a kinematics subject to hostile scrutiny, and one that boasts of a logical pedigree, one cannot be insouciant about the method of "imparting" forces to structures. Later, in the *twin paradox*, it will turn out that the accelerated twin, if it applies the formulas of SRT with similar insouciance, will predict the *wrong sign* of its aging difference from the inertial twin. In that case theorists will tell us that only the testimony of the unaccelerated observer is reliable. In fact, some analysts have gone so far as to stigmatize the accelerated twin as a "bad observer." Others[10] have proposed resynchronizations of clocks as what the accelerated baby should do to avoid being *bad* or soiling its nappy. In any case it is apparent that special exertions (of professional subtlety) are required to keep SRT from contradicting itself where accelerated observers are concerned. It may be inferred that special exertions will be needed in the same cause with regard to the "imparting" of stress-free accelerated motion to a metric standard. So. . .according to SRT, as we know it today, just how is this *imparting* to be done?

Suppose we have our master meter stick and a replica at rest along the x-axis of inertial system S. We want to transfer the replica rod to S', which is moving along the x-direction with constant speed v. To avoid stressing the rod at any moment, we must evidently push (or pull) on each of its particles with a program of force application that is independent of all the other particles. This is true because the particles are longitudinally separated by "spacelike intervals" (SRT terminology for the particles' being out of speed

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c "communication" with each other). What criterion should the force applications meet? It will suffice to consider the simplest case of *constant acceleration g* with respect to the "instantaneously comoving inertial system," as is recommended in the literature.[10] The reasoning by which "constancy" is defined is that a man comoving with the rod and standing on a bathroom scale perpendicular to the *x*-axis should continually register a constant "weight" on the scale. This seems reasonable. The next question is, how are the individual particle forces to be interrelated? A first thought might be to postulate the existence of a common proper time of the rod particles (perhaps the pocket-watch time of the man on the scale). The forces that produce acceleration *g* are turned on at *S*-frame time $t = 0$ and then turned off at some later proper time $\boldsymbol{\tau}_0$ shared by all particles.

That sounds like it ought to work. But it doesn't. Assigning a common proper time to all particles of the rod turns out (according to the Lorentz transformation) to over-constrain the problem, so that the two ends of the rod, when the forces on them are turned off at proper time $\boldsymbol{\tau}_0$ and "coasting" begins, have different coasting-velocities in *S*. This will never do, since it means the rod-length measured in *S* will subsequently change, with stresses sure to be generated in violation of the original requirement of stress-free transport. To fix this, we have to assign separate proper times to each longitudinally-spaced particle of the rod (so that each particle has a proper time *proper to it*), and then to impose the condition that force application is terminated at each particle at a different proper time such that after termination equal "coasting" velocities are exhibited in *S* by all particles. Hence during acceleration there never is an "instantaneously comoving inertial system of the rod," although there always is for any single constituent particle. Note that in SRT there is no reference system, inertial or not, in which *at all times* the rod is *measured* as unstrained (Born rigid, always Lorentz contracted), hence stress-free.

For the *i*th and *j*th particles of the rod the force-termination condition is

$$g_i \tau_i = g_j \tau_j$$

for all *i,j,* where g_i is the constant acceleration of the *i*th particle and τ_i is its acceleration cutoff proper time. When that condition is obeyed, the rod as measured in *S* is calculated, perhaps surprisingly, to be somewhat expanded at the termination moment of the acceleration of the rearmost particle. By the Lorentz transformation, the cutoff at the foremost particle occurs at a subsequent time in *S*. When that later event occurs, the rear of the rod has coasted at high speed in *S* in such a way that the initial expansion has been overcorrected and the expected Lorentz contraction of the distance between front and rear particles of the rod is measured in *S*. The contracted length is maintained thereafter. The rod is therefore declared stress-free and the result is considered to prove the Lorentz contraction of metric standards.

Need I say that if your six-year-old son reasoned in the above fashion you might want to consider special schooling for him? For the reasoning is patently circular, in that it used the Lorentz transformation in calibrating the axes that must already be calibrated in order to give meaning to the Lorentz transformation. (Had we used any other transformation, similar reasoning would have confirmed it.) There is surely small triumph in using what was to be proved in order to prove it. Yet mathematicians noted for their logical minds have enthused[11] over SRT and physicists, of course, have for most of a century gone absolutely gaga, beyond redemption.

Another way of exposing the difficulty is to observe that the necessity of assigning separate proper times to each of the rod's particles "deconstructs" the structure and reduces it to a loose association of independent entities. Absent a proper time *of the rod*, the rod has no "self." Such deconstruction negates any picture of "communication" occurring among the rod's component parts, thereby nullifying any imagined role of body forces, stress, or stress-freedom. In effect, "stress" loses its relevance or meaning. One might just as well define the standard meter in *S* by fixing two non-interacting particles one meter apart. Since two free particles, when accelerated, support no independent physical criterion of mutual stress or lack of it, it becomes obvious that *the Lorentz transformation itself is the only logical progenitor of the Lorentz contraction*. Physics doesn't come into it. The contraction occurs because the contraction occurs. The argument is perfectly circular. As repeatedly pointed out, the Lorentz transformation, or any other coordinate transformation, isn't physics and isn't even semantically meaningful until we get our axes calibrated—that is, until we can assign *numbers* to make the coordinate symbol *values* verifiable. In contrast, physics does come into the issue of classical rigidity versus Born rigidity as the definition of the stress-free condition of extended structures. That issue is one of empirical fact never addressed in SRT nor recognized by its acolytes as worthy of attention. To test the issue experimentally would require very high-speed motion of an extended structure. What can't be tested is politically safe to ignore.

In sum, the purely kinematic way of calibrating axes fails through logical circularity of the attempt to prove the stress-freedom of a transported metric standard. What about other ways not involving material transport? There is, for instance, the notion that cesium atoms or hydrogen, or whatever, at rest in each inertial system provide by their natural wavelengths the necessary length calibration. (Length mensuration is thus elegantly reduced to the counting of wavelengths, an invariant operation identical in all frames.) But if we allege a Lorentz contraction of the wavelengths, it must be recognized that wavelengths are not observable except by the placement of "detectors"—field absorbers, material particles, markers of some kind—at standing-wave nodes or anti-nodes. This recognition leads us back to the previous case, already recognized as circular, of two free material particles one meter apart. (To be sure, we now have to do with Maxwell's equations, rather than SRT, but those equations also depend on prior axis calibration, so the circularity merely extends its ambit to covariant kinematics plus electromagnetism.)

Similarly, to hypothesize that inertial systems have been in motion "forever," and were calibrated in harmony with the Lorentz transformation during some primal era with which we need not concern ourselves, is again circular. . . any other assertion about mensuration properties in such an era being just as plausible, unless we

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favor the Lorentz transformation for the reason that we favor it—*i.e.*, for the reason that our reasoning is circular. Every way we twist and turn a circular pit yawns. Finally, relative to all such conceivable SRT axis calibration schemes, there is also the judgment that, regardless of the success or failure of non-kinematic alternatives, the failure of the *properly kinematic* (metric standard transport) method is *per se* logically unacceptable. Could any number of (hypothetical) successes of subtlety outweigh that one failure of straightforwardness?

Axis Calibration: Length Invariance

Have I softened you up enough that we can now take a look at an alternative to SRT? This is not a trivial question. Most people—and practically all physicists—would require a lot more convincing than I have been able to furnish here before they would open their minds even a crack to SRT alternatives. An open mind is a pearl beyond price; and in today's academic marketplace everything has its price. Never mind—we can proceed without academic science and leave it to catch up in its thousand years.

We have seen that using an invariant time operator in the equations of electromagnetism (replacing $\partial/\partial t$ by d/dt) can produce a covering theory alleged to be "invariant." But this claim is valid only if the space operators in the field equations are also invariant. That could never be true if the Lorentz contraction were physics. The Lorentz contraction is a creature of the Lorentz transformation, whose progenitor is the spacetime-symmetrical covariant Maxwell equations. In contrast, our Hertzian alternative EM theory (which treats time by *total* derivatives and space by *partial* ones) destroys mathematical "spacetime symmetry" already at first order, so we can conceive of (and here in fact propose) a space-time asymmetrical physics in which length is invariant and time is "dilated." The latter phenomenon, for which there is substantial empirical evidence,[12] is immediately and automatically accommodated if in our Hertzian theory we make the further replacement

$$d / dt \rightarrow d / d\tau ,$$

where τ is the proper time of the field detector (the principal actor in any field theory having physical pretensions). Any detector co-moving clock reading is a number, the same for all observers, so $\Delta\tau$ is a *general* motional invariant at all orders. The resulting EM theory I have called[7] "neoHertzian."

How must the force applications to the various particles of our standard rod be regulated to keep its length invariant at all orders of approximation? Einstein to the contrary, this is a physical problem and there has to be some physics put into it. To settle the question just posed, and to inject some physics back into "physics," we need only one *Postulate: Newton's physics is valid at zero velocity.*

Given this, "stress" reacquires its classical meaning (stress-free = strain-free). The requirement of stress-free force application at all times implies that at time $t = 0$ in S, when the rod is at rest, *equal forces* must be applied (simultaneously) to all its longitudinally-separated similar particles. That will produce equal curvatures of all particle worldlines (trajectories). [This is different from SRT, which uses confocal hyperbolas[10] to describe worldlines for constant-g acceleration. This means that at time zero (or any later time) in S, although all worldlines have nonzero curvature, no two of them have the same curvature. Worldlines farther from a unique coordinate origin (located outside the body on the side it moves away from) have lesser curvature. Since force is proportional to worldline curvature, this implies that in SRT at $t = 0$ *different forces* are applied to each longitudinally-separated particle, and these produce different accelerations.]

We emphasize that different forces applied at zero speed would necessarily put a stress on the rod, with loss of its metric standard property, *according to Newtonian physics*. Thus our postulate, together with the requirement of stress-freedom, demands application of identical forces initially, before the rod picks up any observable speed. If the above postulate is physically correct, the Lorentz contraction can never occur; for to occur it must *start* to occur at zero speed. Thus, if we go along with Newton in his domain of generally acknowledged competence, the Lorentz contraction—and with it the Lorentz transformation—is ruled out from the very start.

In its place we have *length* as the spacelike invariant of kinematics. If no contraction occurs in the first instant of force application, it will not occur in later instants, since equal forces produce identical initial worldline curvatures in S, and (assuming spatial isotropy) no consideration of asymmetry can later act to curve them differentially. That is, on successive (hyper-) planes of constant t-time, constantly equal forces applied in S continually produce equal worldline curvatures, with length invariance (classical rigidity) at all times the consequence.

Another way to see this is that, immediately *after* the first instant of force application by the equal-force protocol, the "meter" (defined by unstressed standard rod length), according to Newtonian physics, is rigorously unstressed and uncontracted, hence is length-invariant. Its rigorous invariance, even for an instant, means that notions of simultaneity in S are shared in at least one other inertial system, contrary to Einstein's enlightenment regarding the "relativity of simultaneity." If there is even an instant of failure of Einstein's logic, it must fail generally. We see, in fact, that a new inertial system S^* can be considered to be instantaneously comoving with the just-accelerated rod, and in S^* the rod is momentarily at rest; Newton's physics (by postulate) applies as before, and the same conclusion follows: Again the length standard is invariant. . .and so on for successive increments of the accelerated motion, compounded up to arbitrary relative speeds with respect to the original system S.

For very high speeds it becomes important to reformulate mechanical laws of particle motion invariantly in terms of particle proper time τ, which formally replaces frame time t in Newton's second law, just as in electromagnetism $d/d\tau$ formally replaces d/dt. In both cases this introduces

$$\gamma = 1 / \sqrt{1-(v/c)^2} ,$$

which automatically ensures for real-number measurements that v cannot exceed c. In fact, the invariantly reformulated mechanical law of high-speed motion agrees exactly with Einstein's corresponding covariant law for the single particle. Thus for one-body description the invariant form of Newton's law,

$$d\mathbf{p} / d\tau = d(m_0 d\mathbf{r} / d\tau) / d\tau = \mathbf{F}_{inv} = \gamma \mathbf{F}_{lab},$$

yields

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$$\frac{d}{dt}\left(\frac{m_0\mathbf{v}}{\sqrt{1-(v/c)^2}}\right)=\mathbf{F}_{Lab}\ ,$$

where $\mathbf{v} = d\mathbf{r}/dt$ *and* $d/d\tau = \gamma d/dt$ by the definition of proper time. (Empirical confirmations of this mechanical law—the single-particle experiments previously mentioned—have always been accepted by physicists as the strongest evidence for SRT as physics. Boosters are always boasting about how accelerators couldn't be designed without SRT. That some other theory could yield the same equation of motion, though surely evident to historians of science, is never seriously credited by relativists.)

About the Author

Dr. Phipps was born in Champaign, Illinois on January 26, 1925, the son of an experimental physical chemist. In 1942 he obtained a National Scholarship from Harvard, where he earned A.B. (1945), M.S. (1948), and Ph.D. (1951) degrees.

In 1945-46 he joined the wartime Operations Research Group of the Navy Department in Washington, D.C., then under the direction of Professor P.M. Morse of MIT. There he worked under the supervision of Professor B.O. Koopman of Columbia University, mainly in probability analysis of radar applications to anti-submarine warfare. On completing graduate studies and doctoral thesis experimental work on molecular beam magnetic resonance of hydrogen isotopes at Harvard, under the direction of Professor Norman Ramsey, he returned in late 1950 to operations research (nowadays called systems analysis) for the same MIT-sponsored Navy organization, renamed in its post-war version Operations Evaluation Group (OEG). It was directed by Professor J.J. Steinhardt and located in the Pentagon. In 1955-56 he took academic leave to study elementary particle physics at MIT under the sponsorship of Professor Morse. On his return to Washington (1956) he was assigned to the Naval Warfare Analysis Group in the Office of the Chief of Naval Operations, an offshoot of OEG. During his employment by these Pentagon organizations he carried out a number of field assignments with United States naval forces in Pearl Harbor, Japan (during the Korean war), and the Mediterranean, and also wrote about forty classified studies and memoranda.

In 1960 he joined the U.S. Naval Ordnance Test Station in the Mojave Desert (China Lake, California) as a research consultant to the Technical Director, Dr. W.B. McLean. In 1962 he was appointed Associate Technical Director for Research and Head, Research Department. In 1964-66 he returned to the Pentagon, in the Office of the Director, Defense Research and Engineering as Assistant Deputy Director, Research, under Dr. C.W. Sherwin. His final employment, during 1966-80, was at the Naval Ordnance Laboratory, White Oak, Maryland, then under the directorship of Dr. G.R. Hartmann, where he served as Scientific Advisor to Dr. C.M. Schoman, Head, Advanced Planning and Analysis Staff. This afforded a few opportunities for small-scale physics experimentation.

Upon retirement at age fifty-five, he returned to Urbana, Illinois and set up a small physics laboratory in his father's basement, where they jointly conducted experiments in electromagnetism until his father's death in 1990. These experiments are described in Chapter 7 of *Heretical Verities*, a self-published book he produced in 1987.

Dr. Phipps' professional memberships have included Phi Beta Kappa, Sigma Xi, and, at various times, the Operations Research Society of America and the American Physical Society. He has about fifteen publications in major physics journals and over eighty in dissident physics journals, as well as about fifteen on mathematics and miscellaneous topics.

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The crucial difference between SRT and the invariant alternative is that—since the "worldlines" of the world are put together differently, so that extended structures are described differently—there does in the invariant case exist a *proper time of the rod*. Because of its length invariance, we noted that the rod can be idealized as classically "rigid." It thus possesses a "self," by virtue of this rigidity, and the science of rigid-body mechanics is rehabilitated and is *no longer limited* to low speeds. The "self" of a body exists (and the body is classically rigid) whenever all its particles at all proper times share identical states of motion and thus possess comoving clocks that run at identical rates. (We speak here of general irrotational, not merely inertial, motions. Body rotations are a separate problem.)

We allege that at all times simultaneity is absolute, because when an invariant rod at rest in S' passes an identical invariant rod at rest in S, so that their ends momentarily coincide, if we declare these coincidences simultaneous in S we can define them as simultaneous in S'. This can in principle replace the customary clock-transport and light-signal methods of clock synchronization. (Light signaling can be equivalent to invariant rod coincidences, provided we use Hertzian rather than Maxwellian theory to describe electromagnetism. See references 7 and 8.) Also at all times and speeds in every reference system the rod is measured as stress-free by Newtonian criteria (no new physics!). Contrast SRT, in which we have seen that this *measured* stress-free criterion is satisfied in no reference system. Since length invariance implies absoluteness of simultaneity, the Lorentz transformation and "universal covariance" disappear, and Minkowski space retires into its own "elsewhere."

There are peculiarities about the present ideas that should be noted. All is not roses. For instance, the simplest conception of time dilation pictures it as possibly "caused" by work done on the clock or object (kinetic energy state change)—affecting clock rate just as do changes of gravitational potential energy state. In this case the slowing of an accelerated clock is "real," hence asymmetrical with respect to the unaccelerated clock. This implies, in view of length invariance, nonreciprocity of measured relative velocities among inertial systems. It is by no means certain that a consistent alternative theory lies in this direction: One never knows at the start. Einstein didn't know at the start of his journey: In 1905 he thought that his kinematics concerned idealized (classically) rigid rods; he thought that the group properties of his analysis for one spatial dimension would hold in three-space; he thought that physical inertial systems were permanently irrotational; etc. Every one of these "thoughts" actually introduced a logical inconsistency that had to be removed or plastered-over by the (heroic but unsung) efforts of his friends. (Here's to absent friends: A moment of silence for friends.) Indeed, no theory can succeed without friends.

Nor can the alternative here suggested, which depends entirely on taking Newton seriously in the zero-speed limit. Any who befriend it must build upon the elementary (Newtonian) premise that pushing harder on the back end of a stationary rod than pulling on the front end will a) strain it in the rest system and b) stress it in all systems, and therefore is not the way to keep it a metric standard. Relativists will not hesitate to reject this and to invent whatever *ad hoc* first-order physics is needed to save the higher-order formalism to which they are committed. They are well-practiced in this demanding art, having (over the years) polished it to a

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Foreknown truth, being the only historically proven suppressant of scientific curiosity, is the most effective weapon against science.

high sheen.[8,10] There is nothing to be done for such people. As the most powerful "friend" of Einstein's theory, Max Planck, observed, they just have to be out-lived. Meanwhile, because they ruthlessly control all scientific communication, out-living them will in all likelihood be the thousand-year process we envisioned at the outset. Foreknown truth, being the only historically proven suppressant of scientific curiosity, is the most effective weapon against science.

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COLD FUSION TIMES

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Watch for further Einstein coverage in the next issue of *Infinite Energy.* See list of articles on page 93.

Einstein's epiphany of the "relativity of simultaneity" has to be de-epiphanized.

—Dr. Thomas E. Phipps, Jr.

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New Energy Research Laboratory ✧✧✧ Device and Process Testing Update

Ken Rauen

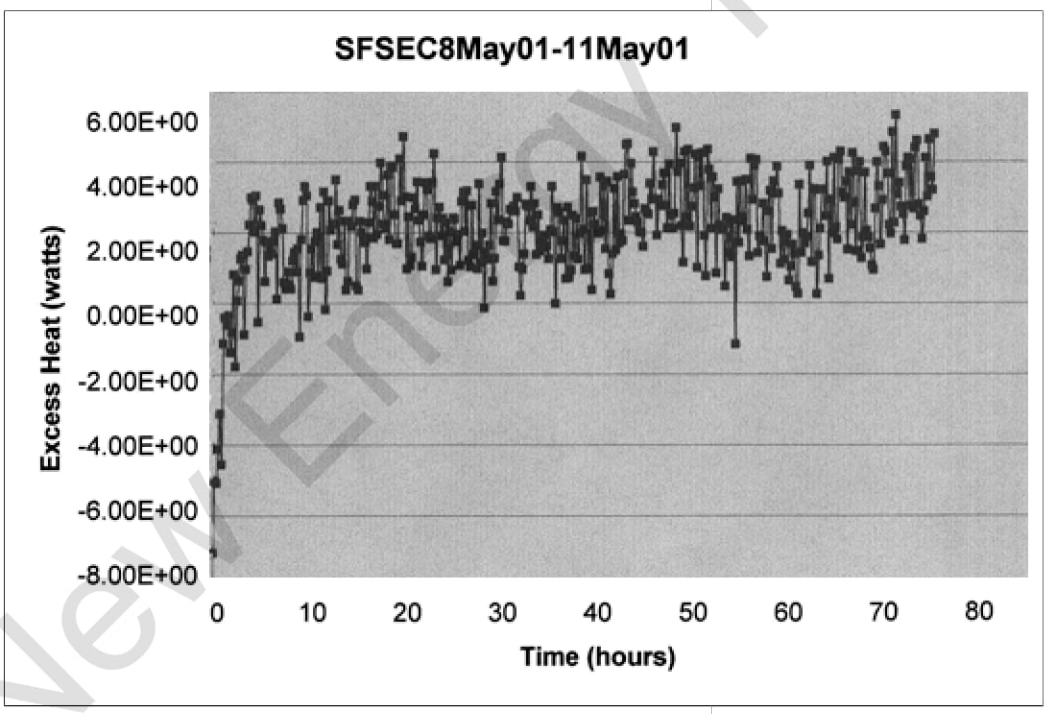
Several sonofusion test runs here at New Energy Research Laboratory with a titanium target were reported in Issue No. 37 to produce excess heat in the 3 to 4 watt range. One produced 3.1 watts after the reactor joule heater had been turned off, which was a surprise to us, because Roger Stringham's experience showed that a reactor temperature around 100°C was necessary and a colder reactor would quench the reaction. As of the last issue, we did not know what to make of this apparent excess heat output.

We now accept the results of that run to be genuine excess heat. Many more tests have been conducted under many different conditions in an attempt to kill the excess heat phenomenon, and it took many tests to find that out. We want to know which conditions produce excess heat and which do not. We also want null tests to be reliable so we can identify the on/off controlling parameters. The more conditions we can identify which do not produce excess heat, the more confident we are that our methodology does not have a systematic error creating an artifact, a false positive signal. A balance is necessary; repeatable zero tests are needed, and of course, repeatable excess heat events are needed also. We now have many of both.

NERL's sonofusion research has been perplexing. Cold fusion categorically has been extremely difficult to reproduce, and that had been NERL's experience until recently. Since Issue No. 37 appeared, and for a short time, we were faced with the novel experience of always finding excess heat, and we could not find conditions that would not produce excess heat except by turning off the ultrasonic oscillator. It was a very strange feeling. We were searching for systematic errors for a month. Finally, we found some conditions which do not produce excess heat and could verify the system calorimetry with more than the joule heaters in the oscillator calorimeter and the reactor calorimeter.

It turns out that excess heat in the 2 to 4 watt range can be produced not only by Roger Stringham's method of ultrasonic activation with applied argon pressure of up to 50 psig and operating just over 100°C, but also by: 1) no applied joule heat and temperatures close to room temperature; 2) argon gas pressure from 0 to 50 psig; 3) ultrasonic power driven with transducer voltages from 600 VAC peak up to 1,500 VAC peak; 4) target or no target in the reaction chamber, and 5) heavy water, normal distilled water, or deuterium depleted (3 ppm deuterium) water. See the display of part of one test run showing excess heat. There seems to be no significant difference in the excess heat by any method, though we have not statistically analyzed any data. Roger believes these *all on* results demonstrate what he wanted with the ultrasonic ring-down pulse; that sweep of operating conditions runs through the unknown but expected "sweet spot" in every test, and in every ultrasonic burst. What puzzles us most is the apparent lack of correlation between excess heat and deuterium concentration. One of our peers in the scientific community thinks we might have a systematic error, since there is no apparent correlation of excess heat with deuterium concentration in the water.

We searched for good zero conditions just as much as for the esteemed excess heat. Nulls were found with: 1) an empty chamber (though not necessarily perfectly dry), and 2) replacing the piezo transducers with dummy resistor loads. The empty chamber has shown us that the Ohio Semitronics wattmeter, which monitors the input electrical power to the oscillator (inside its own separate calorimeter) has a 0.9 watt error, which makes it



A D_2O run at 1500 V peak ultrasonic excitation. A portion of one excess heat run is shown from its data acquisition computer database file. On the graph, excess heat (the noisy trace) has a vertical scale of 2 W per division and the reference center line is 0.0 W.

Device and Testing continued on page 70

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Under Your Own Power

Thanks to better equipment, the number of people venturing off of the commercial electricity grid is growing by a third each year.

Bennett Daviss

Within a decade, the commercial electricity grid will begin to wither. According to the Battelle Memorial Institute, technologies such as microturbines, less-costly wind and solar energy systems, and fuel cells will empower more and more of us to cap our gas lines and pull the plug from the local utility company. Eventually, each home—and perhaps each office and even each appliance—will make its own power in abundance.

For some, the prospect seems like a leap of faith, or perhaps more like a forced march— in part because of the image of off-grid living that persists from the pioneering days of the 1970s: a grimly determined hippie scrimps for months to buy a couple of hugely expensive photovoltaic panels, a few batteries, and quirky power inverter. He lives with a wrench in one hand and a slide rule in the other, holding his patchwork system together with guesses and duct tape. He takes cold showers, reads by the light of a 40-watt bulb, and hoards energy all week so he can watch an hour of television on Saturday night.

But today's off-gridders—with their home entertainment centers, heated swimming pools, and computerized offices—are showing the rest of us that living partly or entirely independent of the grid isn't what it used to be. These days, living under your own power can be almost as easy as relying on utility companies for electricity, and far more liberating.

"I don't want to spend the little free time I have adjusting this and that," says Philippe Habib, who installed a 2.5-kilowatt solar photovoltaic power system on his Mountain View, California home, but still keeps his utility connection. "If this system didn't promise to be reliable and cost-effective, I wouldn't have installed it. A system like mine shows that photovoltaic power is no longer just for tree-huggers."

"Labor?" chuckles Will Beckett in Palo Alto. "The hardest thing I do is watch my electric meter's dials spin backward."

The days of unreliable gear with short lives and limited capacities are gone. "The main misconceptions are that people who live off the grid constantly struggle to keep our systems working, that we deprive ourselves of life's comforts, and that you have to be a rocket scientist to pull this off," says Richard Perez, an off-gridder since 1970 and editor-in-chief and publisher of *Home Power* magazine. "Those need not be true."

Perez's household in Oregon, for example, sports a 19-cubic-foot electric refrigerator, an electric washing machine, a full complement of electric kitchen appliances including a microwave oven, and a television, VCR, and stereo system "that we use a lot," he says.

The house draws power from a 1.5 kilowatt wind generator perched on a 63-foot tower and 78 photovoltaic panels yielding an additional 3.4 kilowatts. More than half of the panels are mounted on tracks that automatically rotate to keep the cells full-face to the sun during the course of the day. "That gives us about 30% more electricity per year than we'd have without the trackers," Perez notes.

The power flows into a bank of deep-cycle lead-acid batteries. Each of four 6-volt cells delivers 840 ampere-hours and weigh in at 325 pounds. They're complemented by another 20 six-volt batteries rated at 350 amp-hours each and weighing a svelte 125 pounds apiece. Energy flows out of the batteries through an inverter, which converts the battery's DC power to standard 60-cycle AC house current. When the batteries are fully charged, an electronic regulator sends incoming current directly to the appliances.

"On a sunny day with a good breeze, our batteries are fully charged by 10 or 11 in the morning," he says. "Sometimes we do things that are anti-conservation, like run our laundry through the washing machine twice. The only way to waste this energy is to not use it."

The magazine's editorial offices are attached to the same system and consume about 75% of the power it produces. When deadlines loom and the scanners, laser printers, fax machines, and five workstations with their big-screen monitors are humming, Perez cranks up the wall-mounted air conditioner to keep things cool.

For all that Perez demands from it, the system requires little in return. Perez tops off the water in his batteries a few times a year, a three-hour chore. In summer, he squeegees the dust from the faces of his PV panels and in winter scrapes off new-fallen snow. Annually, he lowers the wind turbine's tower and takes the turbine apart for an inspection—not a necessity, but he likes to do it anyway. Every decade or so, the batteries wear out and he hauls them to a dealer and exchanges them for new ones.

Wind Turbine, Photo courtesy: DOE/NREL, photo by Bob Thresher.

"Our system requires far less regular maintenance than the average automobile," he says.

Indeed, the banks of car batteries that were part of older solar power systems have been replaced by deep-cycle batteries that live for years, many of which

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use a long-lasting gel instead of water. The industry-wide average efficiency of photovoltaic panels has risen as much as 25% since the early 1980s, according to the National Renewable Energy Laboratory in Golden, Colorado, with the best now converting 15 to 18% of the sunlight falling on them to electricity. During the same period, retail prices have fallen by half. The cost performance of home-scale wind turbines has improved similarly.

"The biggest single improvement has been the invention of the modern inverter in the mid-1980s," Perez declares. "In the old days, good inverters went six months before they blew up." They also didn't make clean power, the kind of current that flows from generating plant in the form of a sine wave. Power coming out of old-style inverters often took the shape of a "modified" sine wave, something more akin to a square wave.

"A lot of AC appliances wouldn't run properly on that modified sine-wave power," Perez recalls. "Some appliances just plain blew up the instant you switched them on. Laser printers, for example, were known as 'fry and die' items."

No more. "The power coming from today's solid-state inverters is of a higher quality— meaning better voltage regulation and lower harmonic distortion—than the power coming out of most people's wall sockets. When we replace an inverter now, it's because a better one has come along, not because the old one failed."

Still, Perez—like almost everyone else off the grid—makes concessions to his power system. Appliances are chosen as much (if not more) for their energy efficiency as their features; the family uses only compact fluorescent light bulbs. "We have a rule of one person, one light," he says. "If you leave a room, you turn off that light and turn on the one where you're going. If it's a sunny day and the batteries are fully charged by 11 in the morning, that's a good time to vacuum the house from top to bottom or do laundry. If it's been cloudy for three days, then you put off doing the wash. If you're off the grid, you watch your system's instrumentation like a hawk and stay flexible enough to make strategic decisions on the basis of what nature gives you."

Being at nature's mercy isn't the only inconvenience. Perez figures his investment in the equipment at about $30,000. "But that's mostly due to the needs of the office," he says. "If we were just a normal household with all of the usual electrical claptrap, we'd need a system about a quarter of the capacity of this one. An investment of no more than $10,000 or $15,000 would do it for most people."

That figure has lured more than 100,000 U.S. consumers away from the grid. Perez's subscriber list includes about 49,000 people who report having cut the utility cord. From data supplied by retailers and manufacturers, Perez pegs the total number of off-grid systems nationally at about 180,000—a cadre that's growing at about a third each year, according to sales figures from suppliers and equipment installers.

There are two chief reasons why people make the choice to move some or all of their electric load off the grid. One is a matter of practicality, the other of principle.

The majority of those with no grid ties buy property beyond the reach of installed electric lines. (The absence of power makes land much cheaper.) Currently, U.S. utilities can charge as much as $50,000 a mile to run a wire to an unconnected site. "The general rule is that a home renewable energy system becomes cost-competitive when you're a quarter-mile or more from the existing grid," says Perez—whose local electric company planned to charge him $285,000 to run a power line the six miles from the nearest pole to his Oregon mountainside property.

For those who live amid the grid, renewable energy systems offer a way to live out a conviction. "We wanted to walk the talk," says David Blittersdorf, "and show that renewables are workable now."

Blittersdorf has put about $57,000 (and much of his own labor) into a comprehensive renewable energy system at his home in Hinesburg, Vermont. A 10-kilowatt wind turbine provides about 55% of the power for his 3,000-square-foot-home; another 5 kilowatts comes from 500 square feet of solar panels arrayed on his roof. The system provides more than 90% of the energy he needs to heat a swimming pool and hot tub, run the household's all-electric appliances including a washer and dryer, and charge his electric lawnmower and the electric scooter he rides to work when weather permits. Moving off the grid entirely would have meant installing a battery bank and a back-up generator, extra expense that he chose to avoid. "The grid is our battery," he says.

Still, there's a financial reward for Blittersdorf's good environmental behavior. Vermont and thirty-two other states have so-called "net metering" laws that require utilities to either credit consumers for, or buy outright, the power they make from renewable sources. (Blittersdorf's local utility will buy his power at prevailing wholesale rates or credit his production against his utility bill at full retail prices.) But even with the utility as a customer, he figures the payout period for his system at twenty-five to thirty years at today's retail electric rates.

Other states are more generous. California, in the throes of its electricity crisis, now pays for half the cost of renewable home energy systems; other states have more modest "buy-down" or tax-credit programs. In those locales, people who send power back into the grid are doubly blessed.

"On a sunny day, my electric meter spins backward from about 10:30 in the morning until 3:30 or 4 in the afternoon," says Will Beckett, who mounted a 4-kilowatt photovoltaic array atop his home in Palo Alto. "If the price of electricity here goes from the 6.5 cents per kilowatt-hour we pay now to 10 cents as predicted, that would cut my payback period from thirty years to about twenty."

Most off-gridders are comfortable taking the long financial view, even with fuel cells and even more advanced technologies on the way.

"Fuel cells that will run on hydrogen still have to get their hydrogen from someplace," says Blittersdorf. "Researchers have talked for years about using offshore wind turbines to make electricity, then using the electricity to strip hydrogen from another source. The hydrogen could be shipped or pipelined to anywhere in the world." Already, home-size units are being tested that can extract hydrogen from water.

Until the days of free energy arrive, even fuel cells will need external energy sources. The best one, many advocates agree, will remain renewable energy. "Wind and solar power are here now, they work, and they're cost-effective," says Blittersdorf. "They're going to be a bigger and bigger part of the energy solution."

Bennett Daviss covers energy, science, and technology for several national publications.

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The Bush/Cheney Energy Plan

John McClaughry*

Late in May Vice President Dick Cheney unveiled his working group report to the President on a National Energy Policy. The report is a comprehensive, factual, hard-headed examination of current U.S. energy use and the options for the future, in an exceptionally fine presentation.

The thrust of the report is, not surprisingly, producing more energy. The report does pay its respects to the merits of conservation and efficiency, and makes a number of recommendations for improved consumer price information, appliance efficiency standards, and fuel efficient hybrid vehicles. But it is clear that cost-effective conservation cannot take the place of very considerable increases in energy production for our energy-drunk society.

To get increased production, the report recommends new oil and gas recovery technology, incentives for deep gas and offshore drilling, new pipeline and high voltage transmission construction, and a host of regulatory changes to encourage coal, petroleum, and nuclear energy production. This part of the report has been the target of environmentalist objections, because it calls for opening drilling on the so-called 1002 area of the Arctic National Wildlife Reserve. This is largely a political and symbolic issue, since the 2000 acre drilling footprint in 11 million non-wilderness acres of Arctic tundra will have no conceivable adverse environmental effect. Whether the 10 billion barrels of oil the 1002 area is believed to contain will ever be extracted and shipped to market, and whether anyone will want it at the price at which it will be offered, are questions for the future.

Of greater interest to *IE* readers are the sections dealing with "alternative" energy sources, which the report defines as transportation fuels other than gasoline and diesel, distributed generation by microturbines and fuel cells, and "future sources, such as hydrogen and fusion."

The first group includes ethanol and biofuel alternatives to petroleum, plus LNG. The report advocates continuing the current ethanol fuel tax exemption, which is equivalent to a 60 cents per gallon subsidy. This has to be one of the most egregious corporate welfare programs in existence. It benefits corn farmers but particularly the Archer Daniels Midland Corporation (ADM). ADM uses corn to make ethanol, for which there would be no fuel market without the subsidy, and distillers dried grains, which are dumped abroad as cheap animal feed.

Distributed generation has intriguing possibilities, but is presently limited by the corridor infrastructure (pipelines, rail) available to bring gaseous fuel (natural gas or hydrogen) to the point of generation. In addition, there is no universal standard for grid interconnection for distributed generation or net metering; zoning laws may block siting of units; and high initial costs and tax disadvantages (compared to utilities) discourage their use other than as emergency backups.

"In the long run," says the report, "alternative energy technologies such as hydrogen show great promise." The report envisions hydrogen production from the combustion of traditional and renewable fuels, and in fuel cells. It supports continuing the DOT fuel cell transit bus program, and expresses the (probably vain) hope that high fuel cell costs will come down dramatically to permit widespread use.

Four paragraphs are devoted to hot fusion: They conclude by saying: "The technological advances experienced over the past decade and the advances yet to come will hopefully transform the energy sources of the distant future." Hope springs eternal.

One word that never appears in the report is "patent." As *IE* readers are painfully aware, the U.S. Patent and Trademark Office has set its official face against anything that can be construed (by establishment scientific critics) to be "cold fusion." Even the allegation that an application represents "cold fusion" has proven sufficient to block the patent. This has happened to Dr. Randell Mills of BlackLight Power (BLP), despite his vigorous protests that his hydrogen orbit shrinking technique has nothing whatever to do with fusing nuclei. BLP was awarded its blanket patent in February 2000, after four years of prosecution, but its next five applications relating to the actual hydride compounds were abruptly pulled back on the eve of issuance. BLP is currently in the D.C. Circuit Court of Appeals protesting PTO's arbitrary and unlawful denial.

It would be too much to expect that a new administration would endorse or even address in its energy policy such controversial and new energy techniques variously called cold fusion, magnetic power, zero point energy, aetheric power, or hydrino energy. Doing so would call down the wrath of orthodox science and the political influence of the government funded laboratories. Perhaps as the body of supportive data for new energy techniques accumulates there will be a corresponding rise in interest in the Bush Administration. Let us hope so.

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If you would like to contribute articles (1,500 words or less) on renewable or conventional energy, please submit them for consideration to:

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Consequences of the Null Result of the Michelson-Morley Experiment:

The Demise of the Stationary Aether, the Rise of Special Relativity, and the Heuristic Concept of the Photon

Paulo N. Correa and Alexandra N. Correa*

Abstract

The extant edifice of electromagnetism owes its inception in the early twentieth century to a profound modification of Maxwell's nineteenth century electromagnetic field theory, first prompted by the null result of the Michelson-Morley (MM) experiment and condensed in what Einstein termed the Special Theory of Relativity (SR), and then complemented by Einstein's light particle hypothesis and the Planck distribution that introduced fundamental discontinuity into blackbody radiation.

SR did not so much introduce new parameters, as it displaced the sense of what Poincaré called the "Lorentz transformations." Whereas these were intended as mathematical artifices employed to salvage the classical theory of an imponderable but stationary aether, they would gain a physical sense with Special Relativity (SR). This displacement most noticeably precluded what Reichenbach once called a Physics of Time.

The first principle of SR posits that there is no absolute motion referenced to an unchanging frame of Space, since all translatory motion is relative to an observer at rest in its own inertial frame of reference. As the covariant complement to the contraction of length in the direction of motion is the objective dilation of time, SR is constrained to "spatialize Time" by reducing it to a length of Space. Subsequently, once all lengths are treated as covariant intervals, the simultaneity of events is no longer invariant, nor, for that matter, is the notion of spatial coincidence. Synchronicity is considered to be an actual impossibility, and the function for a spatio-temporal continuum remains dissociated from any concept of energy, being flattened onto four-dimensional Space-Time.

SR's second principle postulates that the speed of light is a constant for every inertial frame of reference, that is, is the same in all directions and for all observers, and independent of the motion of the source of light or the motion of the receiver, for as long as we are considering solely "substantial translatory motions." SR's position in this respect is somewhat paradoxical: one can say that it satisfies Machian relationism by positing all electromagnetically valid observers as being at rest in inertial frames of translation, their *speeds being all relative and none absolute*. But with the second principle, it explicitly recognizes some form of *absolute velocity*, an *absolute speed* of radiation *in vacuo* which is *constant* for all inertial frames in "sufficient translation."

Where SR had proposed that one should conclude from the MM experiment that there is no stationary aether, and that the propagation of light is independent from the inertial frame of the observer, Einstein's photon theory proposes a new model where there is no need to take recourse to an aether in order to explain the propagation of light.

The authors propose that, whereas SR was correct in positing the abolition of the *stationary* aether and in postulating as invariant the electromagnetic speed c, its requirements for adoption of the Lorentz transformations rely entirely upon the classical electrodynamic interpretation of the Kauffman-Bucherer-Bertozzi-type experiments. Since there are critical alternative evaluations of these experiments, this is a tenuous foundation for the abolition of synchronicity. Furthermore, the authors propose that Einstein's heuristic hypothesis be taken as factual—the result being that electromagnetic radiation becomes secondary to an energy continuum that is neither electromagnetic nor amenable to a four-dimensional reduction. It follows that the second principle of SR only applies to photon production, which is always and only a local discontinuity. It does not apply to non-electromagnetic radiation, nor, *a fortiori*, to the propagation of energy responsible for local photon production.

> *"It was you [my honored Dr. Michelson,] who led the physicists into new paths, and through your marvelous experimental work paved the way for the development of the Theory of Relativity. You uncovered an insidious defect in the ether theory of light as it then existed, and stimulated the ideas of Lorentz and Fitzgerald, out of which the Special Theory of Relativity developed. Without your work, this theory would today be scarcely more than an interesting speculation."*
>
> A. Einstein, speech at the Pasadena Atheneum banquet, January, 1931.

1. The Michelson-Morley Experiment: The Pivot of the Twentieth Century Aether Wars

1.1. Background of the Michelson-Morley Experiment

"Ether or aether (aiqhr probably from αιθω, I burn), a material substance of a more subtle kind than visible bodies,

The authors propose that Einstein's heuristic hypothesis be taken as factual—the result being that electromagnetic radiation becomes secondary to an energy continuum that is neither electromagnetic nor amenable to a four-dimensional reduction. It follows that the second principle of SR only applies to photon production, which is always and only a local discontinuity. It does not apply to non-electromagnetic radiation, nor, *a fortiori*, to the propagation of energy responsible for local photon production.

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supposed to exist in those parts of space which are apparently empty"—so began the article on the "Ether" written by J.C. Maxwell for *Encyclopedia Britannica,* and O. Lodge's last-ditch stand against Relativity—his book entitled *The Ether of Space*. The age of the luminiferous aether had come to a close because of a little known experiment in the annals of science. The Michelson-Morley experiment (1887) posed, at the turn of our century, an unexpected and major doubt with respect to the wave theory of the stationary luminiferous aether: the apparent velocity of the Earth through the aether was zero. Often it is said that the negative result of the MM experiment is proof of Einstein's Special Relativity (SR), yet how can one invoke an experiment as proof for a theory that was created to explain the unexpected result of an experiment, after the fact? The MM experiment no more proves the correctness of SR than it proves the correctness of other theories that also purport to explain its negative result. But it is undoubtedly here, at the crossroads created by this experiment and the multitude of follow-ups, that all contender theories meet to do battle.

Performance of the MM experiment in its essentials had been achieved by 1887,[1] though it was thereafter repeated and improved upon, again and again, by Michelson, Morley, Miller, and others over a period of nearly forty years, before other experiments—carried out by Michelson, and, independently, by Miller and still others—briefly reopened in the 1920s the whole controversy generated by the original findings. The initial thrust of the experiment was to test the nineteenth century hypothesis of a stationary aether in space. If such an aether existed, it should, in principle, permit determination of the sum-total velocity of the Earth's motion through space; specifically, as was thought at the time, it should permit determination of the orbital motion of the Earth around the Sun.

The initial motivation for the experiment, on Michelson's part, stemmed from the discrepancy between Fizeau's 1859 experiment, which showed an "aether drag" effect in the propagation of light through moving water, and Michelson's own 1881 first "aether drift" experiment which appeared to indicate that there was no displacement of the fringe patterns (in Michelson's own words, "the actual displacement was about one hundredth [the distance between fringes, instead of one tenth], and this [was] assignable to the errors of the experiment"). The MM experiment is therefore a long story of attempts to correct what was supposed to be an error—the 1881 experiment, whose result would be further confirmed by subsequent improved repetitions and was opposite to the one expected, as it came to provide instead the interpretive basis for discarding all nineteenth century aether theories, whether based upon aether-wind or aether-drag models.

All of the conflicting aether theories (Fizeau's and Fresnel's partial aether drag theory; W. Thomson's elastic-jelly model; Stokes' 1845 theory of differential aether drag; Maxwell's electromagnetic field theory, to name the most important) struggling at the time for recognition shared the same positioning of the aether problem—in the context of the undulatory theory of light which had displaced the Newtonian theory of light corpuscles and owed much to T. Young's theory of the "luminiferous aether." Young had postulated that the "lumination" of a body is caused by excitation of an aether, highly rarefied and elastic, which pervades space and matter, all "material bodies" undergoing an attraction for this fluid which permits them to accumulate it "within their substance." Light then would consist of the propagation of an excitation in the aethereal medium that fills space, locally being made manifest by the vibrations of this medium and transmitted at a distance in the form of waves of the same medium.

The context for the domination of the undulatory theory of light must be understood. For over a century, two fundamentally opposed visions of nature and of Physics had divided Continental and British philosophers and scientists: Descartes' theory of space as a *Plenum* of seamless aether vortices, and Newton's theory of the aether as the *Sensorium Dei*, an absolute space of reference for mechanical motion. Newton also conceptualized light as the mechanical motion of corpuscles in absolute space, as if light consisted of bodies hurled across space. Eventually, in distinction from Descartes' notion of a Plenum, Newton would come to regard action over "empty" distances as an indication that the aether was synonymous with the Vacuum, and that this alone defined "absolute space," selfsame and unmoveable, a pure container against which we could measure the absolute speed of any and every motion, including that of light's corpuscles.

The Cartesian-Newtonian split betrays how old the foundations of the twentieth century aether wars are: they date back to the schism in Rationalism at the time of the emergence of Physics, in its classical age. The first serious crack in the Newtonian edifice would come precisely with its corpuscular theory of light as a result of its effective inability to account for the periodic light properties of superposition and interference, the very cornerstones of Young's experiments and of his theory of diffraction of light waves, a theory which Young saw as a mere development of Huyghens' idea of a progressing wavefront. Young's contribution went further, however. He introduced as well the concept that, unlike the longitudinal vibrations of sound waves, light consists of transversal vibrations of the aether, as though the aether were an elastic solid, rather than a fluid. Young could not conclude this from his own experiments with interference and diffraction because longitudinal waves, such as sound waves, also show the same effects. But based upon the results of D. Arago and A. Fresnel's experiment, which showed that one could produce two separate and coherent light beams without interference fringes when a light beam passed through a calcite crystal, Young deduced that light must be a transverse wave because the two planes of vibration of the emerging beams had to be at right angles to each other. He proposed the term polarization of light to describe the phenomenon.

1.2. Rise of the Classical Electromagnetic Theory

Further development of aether theory would come from Faraday's experimental work on magnetism which suggested to him that, in distinction from the luminiferous aether, there was also a magnetic aether composed of magnetic lines of force distributed in space. Faraday proposed that "magnetic propagation" took place at the speed of light, but a synthesis of the magnetic and optical properties of the stationary aether would have to wait for Maxwell's electromagnetic field theory. Maxwell mathematically demonstrated how the aether properties of magnetic and luminiferous propagation were identical and equally explained by his concept of light as consisting of an electromagnetic disturbance propagated through the electromagnetic field. An important consequence of Maxwell's theory is that it pre-

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dicted that any radiative disturbance of the electromagnetic field would have to propagate through Space with a definite velocity, the speed of light c, calculable from "nonkinematic" and "nonmechanical" parameters.

Maxwell's contribution of the electromagnetic wave theory of light consolidated classical Physics into an impressive theory. But it also created much controversy within twentieth century Physics. For some, Maxwell was simply crediting the stationary aether with consistent properties that permitted synthesis of optical and magnetic phenomena. For others, the introduction of the field concept already replaced the classical notion of the static aether. In their defense, the latter often cite how Maxwell's electromagnetic properties (permittivity and permeability) replaced the mechanical properties of mass-density and elasticity of the quasi-solid luminiferous aether. Maxwell himself contended that the electromagnetic field was non-material in nature and filled all space, the constant speed c being solely referred to the space frame of this radiation. However, in distinction from modern treatments of the electromagnetic field, which proceed as if radiation were a substance comparable to, but different from, matter, and able to exist by itself in Space empty of matter, Maxwell viewed the optical, electric, and magnetic disturbances as properties of a pervasive aether medium. Some physicists, like Einstein, later considered that Maxwell's theory marked the actual break with the old Newtonian notion that light was composed of particles of matter in motion across space, and thus that it constituted the beginning of a theory of "continuous fields" governed only by partial differential equations and free from mechanical constraints. This is an important aspect of this entire story, in that it would be Einstein who would reintroduce the notion of light corpuscles, in the form of photons, back into modern Physics, in the context of their compliance with a quantum of action; these being the same photons which de Broglie's theory of "matter waves" would predict to have inertial mass, even if minute. But Einstein would never thereby cease to search for a theory of the continuous field that would satisfy Maxwell's intuition of a non-mechanical field action.

However, in a very real sense, Maxwell's synthesis represented the final development of the theory of the luminiferous aether dating back to Young and Huyghens. In Maxwell's theory, radiant energy transfer was still linked to the properties of an imponderable medium and subject to continuous action. Mechanical notions could not be completely abandoned, and today's "electric permittivity" was called by Maxwell himself "electric elasticity," in order to relate it to Couchy's notion of mechanical elasticity. Moreover, Maxwell upheld the wave theory of light as the basis for the synthesis with magnetism and electricity. This coherent position directly led him to stress that what Faraday conceptualized as "magnetic lines of force" should not be regarded as "mere mathematical abstractions," but rather should be considered to constitute real "directions in which the medium is exerting a tension." Some, like Theocharis in 1983, have suggested that Maxwell's treatment of the electromagnetic field as occupying the space that "contains" and "surrounds" material bodies subject to electric and magnetic conditions, should have had the *dual* result of leading Maxwell *to reject* the theory of the luminiferous aether, and in particular the "aether wind" notion originally proposed by Young, and *to put forth* instead the only view consistent with Maxwell's own theory, specifically, an "electromagnetic aether drag model" based upon the notion that the Earth generates its own "electromagnetic aether field," and thus that the velocity of light constant is referred to the "Earth's geofield" and not to any other frame.

Theocharis' argument is a very interesting one. He reproaches Maxwell for being inconsistent with his own theory and for ignoring therefore the Earth's Coulomb field that actually anchors the Maxwellian electromagnetic waves and remains attached to the planet such that it prevents thereby any possibility of an electromagnetic wind. Denoting all the short-range interaction fields (Coulomb, Ampère, Faraday) as radiative I-fields, and all the long-range electromagnetic interactions (light, X, and gamma rays) as radiative R-fields, Theocharis holds that Maxwell should have concluded that the I-fields, specifically the Coulomb I-field, serve as reference for the displacement of the R-fields, even though Maxwell did not know that only R-fields consist of photons. Effectively, Theocharis' relation highlights how Maxwell's theory was unable to overcome the theory of the stationary luminiferous aether, which had disregarded how the propagation of light requires an electric frame of reference that happens to be connected to the inertial frame of reference of the laboratory by virtue of the generation of this electric frame by the Earth's motion itself.

However, irrespective of any inconsistency, Maxwell predicted that it should be possible to determine the velocity of the "aether wind" with respect to the Earth by using the interferometric methods of Fizeau. He had calculated that detection of a second order effect in the v/c ratio was a likely outcome, and this required a $1/10^8$ resolution to carry out the experiment. It is here that Michelson's 1881 quest begins, as he sought to take up Maxwell's challenge and determine by optical means, and with reference to the undisturbed aether of "absolute Space," how fast and in what direction the Earth is travelling.

1.3. The 1887 Michelson-Morley Experiment

Following Lorentz's suggestion, the set-up of the classical 1887 MM experiment involved a rotating interferometer which would measure the speed of two light beams traveling in orthogonal directions (Figure 1). The intention was to measure the degree to which the observed interference pattern would be shifted as the interferometer was rotated through 90°, and thus calculate both the velocity and the direction of the Earth's motion in space. The essential expectation assumed that, as the apparatus rests on the Earth and

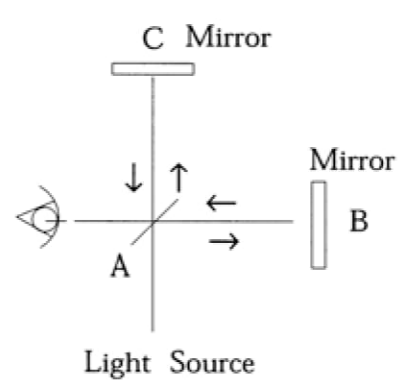

Figure 1. The Michelson-Morley interferometer.

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thus partakes of the motion of the Earth through the aether, and as the Earth moves in the direction of one of the beams—let us call this the A-B direction—the ray that travels along this direction and back from the mirror reflecting it, and thus executes the A-B-A path, must return to the starting point somewhat *later* than the light beam traveling perpendicularly to the first beam along an A-C-A direction. The delayed arrival of the A-B-A ray would then be proven by the displacement of the interference fringes of the two light rays.

If the aether were stationary and occasioned an "aether wind" effect due to the motion of the Earth in Space, and based upon (1) Fizeau's confirmation of the Fresnel formula, (2) the Newtonian theorem of the addition of velocities, and (3) the dependency of the speed of light upon the velocity of the material bodies through which it travels, the expected result of the MM experiment for the speed of light in the direction of motion of the Earth and with respect to the laboratory frame of the observer should obey the Fresnel formula

$$c_{2B} = c_2 + v_1 (1-1/\mu^2) = (c_1/\mu) + v_1 (1-1/\mu^2)$$

The unexpected finding of the MM experiment was its total failure to detect any fringe shift and thus failure to measure any and all of the motional components of the translation of the Earth "through the aether." The average speed of light for a round trip in either arm of the apparatus appeared to be identical within the apparatus' resolution limits, and thus independent of the motion of the Earth through space. No retardation commensurate with the expected result required by the hypothesis of a stationary aether could be found. The results confirmed Michelson's 1881 experiment which had led him at that time, and in disbelief, to conclude what had become apparent: "the hypothesis of a *stationary* aether is erroneous."[2]

We should pause at this point and take stock of the problem. Assuming that the speed of light is a constant only with respect to the undisturbed aether of space, the MM experiment should have detected the ~30 km/s translatory motion of the Earth around the Sun. But it did not, and subsequent repetitions with the same method and down to a resolution of 1.5 km/s failed to detect any aether wind, unlike what was supposed by the theory of the stationary luminiferous aether. This failure would be determinant of the aether wars of the twentieth century, as it would suggest that only Relativity could account for the observed result. Yet, as indicated by Theocharis and already mentioned above, there was an alternative interpretation consistent with a theory of partial drag, such as Stokes' theory, including his notion of an aetherosphere shielding the motion of such bodies as the Earth: if the frame of reference for the propagation of light in the laboratory is taken to be the electric field of the Earth, attached as it is to its inertial frame and being responsible for the Earth's geomagnetic field, no shift of the interference fringes should be anticipated. In a letter to Alexander Bell, on April 17, 1881, Michelson expressed the essence of this view by concluding from his 1881 experiments that "the question is thus solved in the negative, showing that the aether in the vicinity of the Earth is moving with the Earth; [this is] a result in direct variance with the generally received theory of aberration" and the notion of an aether wind resulting from compression of the wave front in the direction of motion.

But the century would not remember these courageous words of Michelson, nor would Michelson repeat them on the occasion of the 1887 experiment and subsequent ones. Indeed, the negative result did not imply that there was no aether filling space; only that the stationary model of the luminiferous aether and Young's aether-wind effect were erroneous hypotheses. Instead of physicists heeding Michelson's early conclusion, the bells tolled for any and every aether theory. It was the entire foundation of Physics for over three hundred years which was shaken to the core. H. Poincaré, French philosopher and scientist, was probably the first one to suggest the need for a new physical hypothesis capable of accounting for how optical phenomena depend solely on the "relative motions" of material bodies with respect to other material bodies, luminous sources, or optical apparatuses, confirming therefore Mach's contention of the relativity of all motion. No absolute motion in space could be detected in principle, there was no absolute observer at rest in a stationary medium. With Poincaré the question of the electric frame which serves as reference for the propagation of light in the laboratory frame had already been reduced, and precociously so, to the question of the reference frame being an inertial one. Only for inertial frames was the speed of light constant, this forming the basis for all relativistic theories. By 1899 Poincaré had enunciated the fundamental postulates of what would later become Special Relativity: (1) the null result of the MM experiment indicates that the aether does not exist; (2) physical laws must be the same for a fixed observer as for one in uniform motion of translation relative to one at rest; and the principle of the "new dynamics" would be that no velocity can exceed that of light.

As E. Whittaker observed in his voluminous history of the theories of the aether, the real inventor of the Principle of Relativity was Poincaré and not Einstein. With Poincaré, an epistemological break occurs with classical Physics which would forever change the conceptual framework of physical theory and condemn the concept of the stationary aether to the dustbin of history. Be that as it may, as Essen has pointed out in his scathing attack on Relativity, the "new dynamics" which Einstein would formalize could never pretend to find proof for its axioms in the MM experiment because it arose precisely to explain the null result of the experiment. The results of the MM-type experiments conform to the notion that none of the translatory components of the Earth's motion can be detected optically or electromagnetically by reference to a stationary aether. They preclude therefore the existence of an aether wind caused by translation-induced compaction of a stationary aether.

1.4. Lorentz's *ad hoc* Hypothesis of Contraction

In the wake of the MM experiment, perhaps no theory ushered in Relativity faster than Lorentz's efforts to salvage

The negative result did not imply that there was no aether filling space; only that the stationary model of the luminiferous aether and Young's aether-wind effect were erroneous hypotheses. Instead of physicists heeding Michelson's early conclusion, the bells tolled for any and every aether theory. It was the entire foundation of Physics for over three hundred years which was shaken to the core.

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the old model of the stationary aether by suggesting that the impossibility of determining absolute motion simply resulted from a change in length that took place for every object in a state of motion. Lorentz's hypothesis of a contraction of length in the direction of motion was solely an *ad hoc* theory which led him to stress his main thesis regarding the fundamental separation of the aether and matter. Only ponderable matter was subject to the contraction hypothesis, and this therefore precluded us from measuring its inertial speed with respect to the imponderable aether of space.

Lorentz's transformation indicated that the length of an object, be it a pencil of light, as measured with respect to a system of reference in relation to which it moves with velocity v, was proportional to the second order ratio $[1-(v^2/c^2)]^{0.5}$. Lorentz effectively proposed what would later become the cardinal point of Special Relativity,[3] viz that mass or length in the direction of motion contracts with increasing speed. With reference to Figure 2 and the MM experiment, specifically, he postulated that the beam in the direction A-B became shorter both in length and in time as it moved against the aether, thus shortening the A-B-A pathway. Hence the A-B-A ray returned just as fast as the A-C-A ray did. In Lorentz's view, the co-contraction of time and of mass or length (extensivity) was a property of the aether itself, the aether exerting "shortening forces upon the moving bodies in such a manner that the differences in the velocity of light connected with motion cannot be demonstrated."[4]

G. FitzGerald had suggested a similar hypothesis in a 1889 letter to Michelson, predicting that the contraction of the length of material bodies would vary by an amount given by the square of the ratio of their velocity to that of light. The Lorentz-FitzGerald contraction, as it became known, would constitute the cornerstone of the theory of Special Relativity, despite its intended objective of salvaging the static aether concept. Integration of the Lorentz-FitzGerald contraction theory with Maxwell's equations would essentially be the work of Poincaré, rather than Einstein's.

To be consistent with the hypothesis of length contraction in the direction of relative uniform motion, there should also be a reciprocal co-variance of time, of flow of time. This is the basis of the fourth equation of Lorentz—as it yields a transformation which will become one of the fundamental tenets of Relativity, both Special and General: the dilation of time proportional to increasing speed.[5] To the aggregate of all four co-variant equations of Lorentz, Poincaré gave in 1905 the name of "Lorentz transformations," which are expressed as follows:

$$x' = (x-vt) / [1-(v^2/c^2)]^{0.5}$$
$$y' = y$$
$$z' = z$$
$$t' = [t-(v/c^2)x] / [1-(v^2/c^2)]^{0.5}$$

as opposed to the old mechanical equations of the Galilean transformations:

$$x' = x-vt$$
$$y' = y$$
$$z' = z$$
$$t' = t$$

Lorentz's transformations were one last ditch attempt at preserving the romantic belief in the stationary aether, as one would *otherwise* have to assume that no proof of the existence of the aether would ever be possible. Akin to the actual impossibility of demonstrating the metaphysical dogma of the existence of God, which is therefore said to be a matter for faith alone, the aether—in a Lorentzian universe—could only *be inferred by its very absence*. Lorentz's contraction of time in the direction of motion was a stratagem to preserve our scientific belief in a God for physicists. But, thanks to Poincaré and Einstein, this would not last long, even though, in a very real sense, just as Maxwell inconsistently preserved the luminiferous theory of an aether wind, so did SR preserve the Lorentzian notion of a stationary aether that escapes detection, to the point that the physicist can go about his calculations as if the aether did not exist, the matter of the aether being relegated into a domain of metaphysical credo as either belief and disbelief both appear to have effectively become phenomenologically compatible and indistinguishable.

> *"The result of the famous Michelson-Morley experiment was a verdict of 'death' to the theory of the calm ether-sea through which all matter moves. No dependence of the speed of light upon direction could be found."*
>
> A. Einstein and L. Infeld, *The Evolution of Physics*, 1938, p. 174.

2. Special Relativity and the Death of the Stationary Aether

2.1. From a Heuristic Hypothesis to Special Relativity

The initial epistemological problem with the relativistic interpretation of the negative results of the MM experiment is that it erased the physical basis for the concept that light waves were analogous to water waves, transversal waves in a medium, waves *of* a medium. If light consisted of waves, these waves had to be waves of something, of something like water, waves *of* the aether. But, if the aether did not exist, *light waves were waves of what?*

At first, Einstein's theory of Relativity did not so much appear to be concerned with an answer to this question, as it was with establishing the basis for the equivalence of all inertial frames of reference in uniform translation. As Poincaré had previously pointed out, this alone would provide the physical basis for the relativity of all motion and the universality of the Lorentz transformations. Einstein's approach was mindful of the possibilities of an eventual theory of "continuous field action," but it would be precisely at the heart of such theory that he would strike, first by proposing that one should conclude from the MM experiment that there is no stationary aether, and thus that the transformations proposed by Lorentz were not mere optical aberrations or, as in the case of Time, mere mathematical conversions, but were actual physical changes; secondly, by arguing that there is no

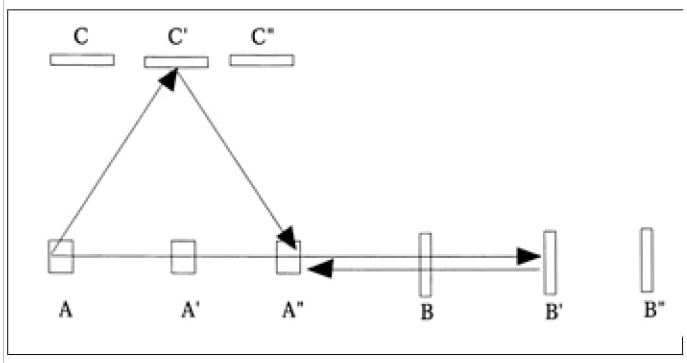

Figure 2. The Michelson-Morley experiment.

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need to take recourse to an aether in order to explain the propagation of light *in vacuo*; and thirdly, outside of the Special Theory proper but yet in a manner designed to complement its framework, by his theory that light was made up of particles (quanta) associated in bundles and emitted with a speed that was invariant for all frames of reference.

Hence, Lorentz would state in 1927: "I considered my time transformation only as a heuristic working hypothesis. So the theory of relativity is really solely Einstein's work."[6] In Einstein's special theory, there is no medium for light to travel "in" or "through," there is no fixed Space frame of reference for the propagation of light, its transmission being always effectuated at the same speed in all directions of any and every uniformly moving frame of reference. It is important to note at this conjuncture that the MM experiment could be "ideally construed" as a test of the Lorentz contraction theory, given that, in the absence of any relative motion between the apparatus and the observer, length contraction with respect to an ideal stationary aether could be said to occur—amongst other alternative interpretations. The same, however, cannot be said with respect to the MM experiment serving as a test for Special Relativity. Such a test would in fact *require relative motion of the apparatus with respect to the observer*, and this is not what the MM experiment involved.

In his later years, Einstein would describe the theory of Relativity as a theory of principles that utilized the analytic method and stuck to empirical observations. The empirical reference for such a statement when it comes to SR can only be that of the MM experiment, and yet all that SR can claim in this respect is that it does not conflict with this empirical result, *not* that it can validate its analytical method by the MM experiment. Einstein enunciated the two critical principles of SR as being the equivalence of inertial frames of reference and the constancy of the velocity of light in a vacuum. Few realize the paralogical assumptions that each of these two principles entail.

The *first principle* of SR, which Einstein traces back to the ancient Greeks, requires that the motion of every material body be referred to another body which, in the context of Galilean-Newtonian mechanics, serves as a spatial system of coordinates. This principle can be formulated as: there is no absolute motion, no absolute observer at rest in the inertial frame of Space, since all motion is relative to an observer at rest in an inertial frame of reference. There are plenty of both apparent and hidden assumptions in this principle, and even more when it is wielded by SR.

1) The first hidden assumption is that the Galilean system can be mathematically *and physically* extended to a flat Minkowski four-dimensional Spacetime continuum, where Time is first reduced to a length of Space and next all lengths are treated as co-variant intervals. The net result is that SR's concept of the continuum corresponds essentially to a 4D pseudo-Euclidean Space. The assumption of a Minkowski Spacetime solution implies therefore a spatialization of Time.

2) The second hidden assumption is that SR holds that simultaneity is impossible because it is relative to "relative position." As there is no "Space at a Time," the simultaneity of events is no longer invariant, nor for that matter is the notion of spatial coincidence. The notion therefore of a spatio-temporal location itself becomes a "fuzzy" one.

3) Diachronic separation of events in Time has no absolute reality, being exclusively relative to the state of motion of an observer.

4) The definition of the valid frames of reference rests on the definition of inertia, or the uniform motion of inertial frames, and inertia is essentially defined in a Newtonian fashion, by the resistance to acceleration or to change in direction. This implies that a valid reference frame must be substantially free from rotation, twisting, and acceleration. That is what Einstein meant when he formulated his basic principle of SR as: "Every law of nature which holds good with respect to a coordinate system K must also hold good for any other system K' provided that K and K' are in sufficient movement of translation."

It is this tenet which serves as foundation for the relativistic equivalence of all inertial frames of reference. Yet it is also clear that such a principle makes some further axiomatic suppositions:

5) The first implicit reduction is that *all translatory motion* not subject to changes in acceleration *is inertial motion*. Inertia then, is the property of mass in translation.

6) Therefore, the valid SR frames of reference are necessarily limited to those which are axed on mass-energy that has constant kinetic energy associated with it.

7) Finally, implicit on all this, but unmentioned until the advent of General Relativity, there is a definite assimilation of inertial motion to the gravitational motion of free fall, as the latter constitutes the very model for substantial translation devoid of rotary components. This hidden axiom postulates that inertial motion and "gravitational" motion are one and the same type of motion.

The *second principle* of SR is that the speed of light is a constant for every inertial frame of reference, that is, the same in all directions and for all observers, as well as independent of the motion of the source of light or the motion of the receiver, for as long as we are considering solely "substantial translatory motions" in "empty space."[7] The constancy of the speed of light means that "in a system of reference rotating with respect to an inert system" (*e.g.* the axis of the Earth) "the laws of disposition of rigid bodies do not correspond to the rules of Euclidean geometry on account of the Lorentz contraction," to quote Einstein's words in "Sidelines on Relativity." Einstein himself acknowledged that this principle was implicit to "the Maxwell-Lorentz theory of electrodynamics," this being perfectly sufficient for him as a basis for its validity. Hence, one can say that SR satisfies Machian relationism by positing all electromagnetically valid observers as being at rest in inertial frames of translation, their *speeds being all relative and none absolute*. But SR recognizes some form of *absolute velocity,* strictly speaking not for a particle-photon, but for light waves in a vacuum

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and in the absence of matter. Hence, by virtue of the reference to an *absolute speed* of radiation *in vacuo* which is *constant* for all inertial frames in "sufficient translation," we can determine the *relative speeds* of translation using any inertial frame as their reference.

It is apparent that Einstein's contribution, beyond the theories of Lorentz and Poincaré, was not so much the enunciation of new principles, but the axiomatic synthesis of principles which up until SR seemed to be logically incompatible. This reconciliation of opposites, as Einstein himself once described it, led to a "new kinematics" which proscribed any form of simultaneity and, at the same time, prescribed a time-dilation function. The Time transformation previously suggested by Lorentz to be a mere phenomenological barrier to our detection of nature, becomes with SR a physical reality that permits the existence of separate times ruled by the ratio of the inertial speed of their rest frames to the speed of light. Einstein's procedure has in fact substituted any observer "in sufficient translation substantially free from rotation" for the stationary aether of nineteenth century physics, with the result that motion, with respect to the inertial frame of the observer, retains all the elements which it had when considered as motion with respect to absolute space, the inertial frame of the aether. This then is taken as the necessary price one must pay in order to consider that all valid observers are absolutely in a state of translation!

SR's second principle, relating the absolute speed of light as invariant for all frames of matter in translation, indicates that neither in electrodynamics nor in mechanics are there properties "corresponding to the idea of absolute rest." But since this invariance only applies to inertial frames, it is in fact made relative to a state of motion—that of translation. Therefore, there exists this deep connection behind the apparent disconnection of the inert frame of moving matter and the invariant propagation of light: if the reference is mass in translation, the frame is validated. This link between electromagnetic energy and the inertial frame of mass is complementary of the principle of equivalence of mass and energy, such as Einstein understood it. But here, too, we have a tremendous paralogical leap of faith, one that requires our understanding of the equivalence of mass and energy to be a one-way street: mass is equivalent to electromagnetic energy, because it converts into the latter. But when the physicist is concerned with the reverse process, the only empirical evidence he can adduce is the relativistic interpretation of an increase in mass for particles accelerated to near-luminal speeds, which he obtains by applying c as a constant (petition of principle), and which appears to confirm that (1) kinetic energy is electromagnetic energy, and (2) electromagnetic energy can be converted into mass. Yet, the electromagnetic mass-energy equivalence thus "adduced" by effective petition of principle is unable to explain the creation of matter, *i.e.* the creation of inert mass-energy; it is only capable of explaining the *addition* of mass, and that, too, only in principle. The obvious assumptions, in this respect, subjacent to SR's second principle are that: (1) kinetic energy can only exist in association with, and with reference to, inertial mass; (2) all energy is electromagnetic energy.

Philosophically speaking, Einstein's démarche is tantamount to overcoming the old "thesis" denied by the MM experiment (empirical antithesis) by recuperating the old "thesis" under conditions that make it appear to be a "new" thesis, a "synthesis." But the "old" shines through the "new," as it is still to the idealized notion of a stationary state of matter ("at rest in its own inertial frame of motion") that all motion is referred. Einstein had just replaced the "inertial frame of reference of the aether" with the inertial frame of reference of any material body in a state of substantial translation. Some have argued that Einstein's observer is a microphysical reality beyond human perception, void even of any real senses. As an observer, not only does he appear to lose the sense of real irreversible Duration (through the mathematical fiction of *a Time in extension*), but what is still more fundamental for a microphysical perspective, he loses the sense of simultaneity because of his "heavy" condition, as if his perspective was forever that of inertial mass in free fall.

The price for SR's synthesis was therefore the introduction of "actual" time-dilation; thereby, the timing clock of the observer's laboratory ceases to be the actual rotation of the Earth (which can even be ignored from the viewpoint of SR), to become the clock of the propagation of light. In doing so, Special Relativity treats the spatial figures of light lines in Space as lines of Time, as if they were actual lines of time.[8] From Lorentz's heuristic time transformation, Special Relativity extracts the transformation of length, the contraction of extensity, and thus of matter, as a function of addition of inertial mass with increasing speed:

$$l' = l\,[1-(v^2/c^2)]^{0.5}$$

and its corresponding time-dilation functions for two interchangeable frames of reference:

$$\Delta t' = \Delta t\ \ [1-(v^2/c^2)]^{0.5}$$
$$\Delta t\ \ = \Delta t'\ [1-(v'^2/c^2)]^{0.5}$$

Hence, in the context of the Michelson-Morley experiment, the two light-time lines, with longitudinal and transversal paths, must remain identical with respect to the length of the complete circuit (emitted and reflected arms). And it was to obviate the objection that Newcoomb had already addressed in 1881 to the Michelson experiment, that "when a ray returns on its own path the retardation in one direction is compensated by the acceleration in the other,"[9] and hence that there is no way to determine whether the velocity of the light beam along the AB direction was greater or smaller than in the BA direction, that Einstein put forth the Special Theory's view of the Relativity of simultaneity. It is this component of Relativity that will fragment Time and transform Duration into a fourth dimension of Space, the *sine qua non* of replacing the aether by the observer at rest in his inertial frame. In Einstein's view, simultaneity close by, in the same frame of reference, is not the same as simultaneity at a "distance." Michelson could not determine the velocity of light in a single direction, *e.g.* in the emittive direction alone, he had to reduce himself to the average of the velocity in both arms, emittive plus reflective. But Einstein argued that determination of the velocity in a single direction would be useless because the difference would appear as a null one: even if we were to use two clocks at different points, we would still have to determine whether or not the clocks would show the same values at the same time. In other words, simultaneity itself is relative to the selected inertial frame. As Reichenbach described it: "We find ourselves in a vicious circle: in order to determine the simultaneity of distant events,

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we must know a velocity; and in order to measure the velocity, we must be capable of judging the simultaneity of events separated by distance."[10] This is the very fundamental *fuzziness or undecidability* of relativist science.

Provided that the speed of light be considered a constant, we may define simultaneity consistently, *without ever being able to verify it*. Such is the tenor of Einstein's Special Relativity. And provided we define simultaneity in the same relative way as we defined the speed of light constant for all inertial frames in substantial translation, we may introduce into the universe of experience a method to determine a spatio-temporal order. As Reichenbach also succinctly puts it: "Einstein's theory of simultaneity has a presupposition without which it could not be maintained: it is nothing other than the assumption that no velocity greater than the velocity of light can occur in nature."[11] As long as we do not assume that electromagnetic energy may travel faster than the speed of light constant *in vacuo*, the relativist hypothesis applies. As we shall see ahead, with the generalization of the relativistic principle to the "gravitational field," the light speed limit will be applied to both electromagnetic and gravitational waves, with no other basis than an axiomatic one.

Finally, the change of mass due to relative velocity is given by SR as

$$m' = m/\ [1-(v^2/c^2)]^{0.5}$$

which means that mass increases with the velocity of a body relative to an outside observer's system of reference. Even in relativistic terms, this statement may have solely a heuristic value, in principle telling us nothing about actual addition of inertial mass. In fact, aside from the obvious unwitting contribution to SR on Lorentz's part, both in terms of electrodynamics and the contraction hypothesis, classical theory had already made a distinction between *proper mass*, which was velocity-invariant, and *electromagnetic mass*, which was velocity-dependent, and this in fact was the pre-relativist interpretation of Kauffman's original 1910 results with particles accelerated to near-luminal speeds.

2.2. SR and the Einstein-Planck Theory of Quantum Discontinuity

In 1901, Planck proposed the notion that electromagnetic radiation (such as light or heat) may be emitted by a black body in small, discrete steps, rather than continuously (note that Planck's original theory did not involve quantization of absorption, a question which was only resolved later, in 1913, with Bohr's model of the hydrogen atom). In 1905, following his studies of the photoelectric effect, Einstein extended Planck's notion of discontinuity by proposing the concept that light had a granular structure, consisting of discrete elements, quanta, later termed photons. While the classical Rayleigh-Jeans distribution had predicted infinite energy in the radiation field for low-intensity (high frequency) black-body radiation, Einstein mathematically demonstrated that this prediction was in error due to the fact that the classical distribution had not taken into account how the entropy of radiation behaved at low intensity (high frequency) as the entropy of particles, that is, how it behaved differently from the high-intensity (low frequency) entropy of waves.

From the same set of equations, Einstein then showed it was possible to produce both the continuous Rayleigh-Jeans distribution and the discrete Planck distribution. In treating high frequency radiation as bundles of discrete elements, the photons, Einstein was demarcating the singularities of the field, its particle-like behavior, from the energy fluctuations observed at high intensity resulting from the interference of waves, thus effectively deriving the Rayleigh-Jeans distribution from Planck's law itself, to the detriment of the "fluid aether" theory of Jeans.

This shattering of the classical perspective of a continuum of radiation inherent to the very notion of field would introduce a profound theoretical break that also marked the beginning of the quest for a unified field theory—which so occupied twentieth century physicists. But until today, the fragmentation of the notion of field by quantum singularities has effectively preempted any successful theory of a continuous field. Einstein would go on to search for a set of non-linear field equations that would reduce to Maxwell's electromagnetic field equations at high intensity, where statistical processes predominated, but would present "discontinuities as singularities of the field" at low intensities. With Einstein's concept of discontinuity, electromagnetic theory not so much stood on its own feet, as it was stood on mere phenomenological grounds, due to statistical considerations: "Although Maxwell's theory is not applicable to elementary resonators, the *average* energy of such a resonator in a radiation field is the same as that which one would compute from Maxwell's theory."[12]

Even though it is apparent that, until the end of his life, Einstein was not satisfied with the answers provided by quantum mechanics or even wave mechanics, because a theory of continuous fields remained effectively precluded, there is no denying that SR's concept of Spacetime as an empty and flattened container (very much a Euclidean inheritance), as well as its assimilation of Time to this geometric concept (arrived at by adopting Minkowski's notion of Space-Time), melded easily both with his and Planck's theory of electromagnetic field discontinuity, as well as with the finding that at high frequency the entropy of electromagnetic radiation behaved like that of particles.

Many have sought to relate Einstein's thought to that of Descartes, or even more tenuously, to that of Spinoza. But the real connection to be made in this respect is with Newton, and not Descartes or Spinoza, as Einstein and Infeld pointed out.[13] For, just as in Newton's theory we have an absolute 3D-Space which is an empty locus (the Vacuum) and a pure geometric form populated by material light corpuscles hurled at a distance across space, so is Einstein's Space-Time a pure four-dimensional geometric form, void of energy where it is void of matter, and populated by bundles of photons, that is, effectively populated by discrete light corpuscles. In this quantum-relativistic scenario, once again,

As Reichenbach described it: "We find ourselves in a vicious circle: in order to determine the simultaneity of distant events, we must know a velocity; and in order to measure the velocity, we must be capable of judging the simultaneity of events separated by distance. . .Einstein's theory of simultaneity has a presupposition without which it could not be maintained: it is nothing other than the assumption that no velocity greater than the velocity of light can occur in nature."

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as in Newton's scenario, we do not need to worry about what it is that waves wave *in*. Light may have wave-like properties but is quantized as a function of increasing quantum frequency. In a real sense then, Einstein's theory of discontinuity returns a re-interpreted Newtonianism back to the throne of official physics. In the old Atomistic tradition of science, Einstein's photon—the atom of light—stands in a long line of atomistic quests for the ultimate elements of matter: the atom for chemical reactions, the electron for electric reactions and the photon for electromagnetic reactions. Even if Einstein was not and could not be satiated with this quantum-Atomistic picture, he could neither set it aside nor reconcile it with a theory of energy continua.

One may deepen this link further—as it presents the roots of that other great dilemma of twentieth century Physics: are there wavicles? What are particle-waves or matter-waves—are they just misnomers? The very notion of a wave-particle couple, a form of synthesis of Cartesianism and Newtonianism, goes back to the nineteenth century theories of the luminiferous aether, if not to Newton himself, who actually had tried to conjugate the notions of waves and corpuscles. William Thomson, staunch defender of the stationary aether concept, would write: "You can imagine particles of something, the thing whose motion constitutes light. This thing we call the luminiferous ether."[14] In fact, the notion that such particles might be material, in the sense not of their physical reality, but in the sense that they might partake of some of the properties of ponderable matter or inertial mass, though a specifically mechanistic view, is not however foreign to the theory of the luminiferous ether. The differentiating concept implicit in Einstein's concept of the photons is the quantum of action later denoted by the Planck constant h, that is—the new idea of discrete or discontinuous energy emissions: that light-waves consisted of *discrete* particles, that discontinuity was quantized, *was* the new insight which *in practice* did away with the old notion of the aether. The notion that the energy stored in any resonator does not vary by gradual changes, but by whole units of energy, became the backbone of a modern physical theory that, at the limit, could describe physical forces and interactions without taking recourse to the Romantic notions of absolute Space, absolute Time, absolute speed, and absolute motion. But does this mean that modern Physics had shed all metaphysical prejudices?

To keep this problem in perspective, we should realize the import of Einstein's designation of the "Maxwell-Lorentz electrodynamics" as the basis for a "new kinematics." For the very notion of particles of light did not necessarily entail that all adherents of the luminiferous aether which shared with the Newtonians this concept, considered these particles to be *inert mass bodies*—as the corpuscular theory of light held. Indeed, here is where the cleavage between the mechanical theory of Newton and the undulatory theory of Young becomes the starkest. The champion of this realization must be found in Maxwell, to the very extent that he searched for the non-mechanical and non-material properties, the electric and the magnetic properties, of light. But with Lorentz a new proviso arises, that length contraction and associated transformations must refer solely to the motion of ponderable substances, and *not to the motion of the imponderable aether.* Einstein, having operationally discarded Lorentz's stationary imponderable aether with the SR theory, could now apply a relativistic treatment of Lorentz's electro-mechanical principles to electromagnetic radiation, by assuming that all charge is associated with inert mass—even though the electrodynamic effect caused by different masses of charge carriers is entirely ignored by Lorentz's electrodynamics of ponderable matter. One might object to this contention of ours by arguing that, initially, Einstein's demonstration that light must assume resonator energies restricted to integral multiples of $h\upsilon$, was perfectly compatible with the notion of *massless photons,* and hence, that it was not until de Broglie's formalization of matter wavicles that the modern concept of a mass-carrying photon emerged. But the fact is that, in SR, no form of energy is exempt from relativistic constraints (c being a constant is the form of the constraint such as it applies to photons in their inertial frame of reference). It is therefore implicit that light itself, photons as constituent elements of light, must have a quantum of inertial mass, no matter how tiny. It is in this very sense that the concept of mass-energy is today applied to all forms of electromagnetic radiation, and that the bending of light in gravitational fields is explained: the deviation of light in the Doppler effect, said to be caused by the curvature of nearby Spacetime, is simply a geometric expression for the supposed inertial mass of the photon in a gravitational force-field.

We shall see how this link of the photon to inert mass becomes critical from the vantage point of Einstein's enunciation of the theory of General Relativity (GR). By acquiring mass, the Einsteinian light particles (the photons) made a full return to the Newtonian universe of mechanical forces between units of matter, atoms and granules of ponderable mass.

> *"Are not gross Bodies and Light convertible into one another. . .?"* I. Newton, *Opticks,* Ch. 8.

3. The Primacy of Inert Mass and Electromagnetic Energy and the Question of a Non-mechanical, Non-stationary Aether

If the equivalence of inertial mass and electromagnetic energy was ascribed by Einstein to his SR theory, nevertheless the relation $E = mc^2$ had already been proposed by Jeans in 1904, in the context of Jeans' own aether theory of transmutation of mass into energy, and still before Jeans, by Poincaré in 1900 (how ironical it is that Einstein is most remembered by this relation which he did not even discover!). But Einstein explicitly wanted it to be understood as a logical consequence of the first principle of SR:

> The most important result of the special relativity system concerned the inert mass of a material system. It became evident that the inertia of such a sys-

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tem must depend on its energy content, so that we were driven to the conception that inert mass was nothing else than latent energy.[15]

But how are we to understand inert mass as latent energy? It is only latent from the viewpoint of a theory that pretends that all energy transfers are electromagnetic (and at that, only in the epiphenomenological sense that the average resonator is Maxwellian) and which, furthermore, holds as exclusive the viewpoint of electromagnetic energy converted from disintegrating mass. But as inert mass, *qua* mass, what exactly is the form of the energy that yields the effect of the identity of inert and heavy or weighty masses? That is the question that neither Relativity nor quantum or wave-mechanics ever was able to answer. The problem ties in directly with the lack of understanding of the physical processes responsible for the creation of mass-energy. One may well liberate photon energy from the disintegration of matter, or mass-energy, but can one hold as a fact that, if all mass is congealed energy, so is all energy liberated matter? SR tells us that we must. But must we? For how are we to prove that it is from electromagnetic energy that mass-energy arises? If pair-creation were the only way to produce electrons, why does nature present us with such an unbalance between negatrons and positrons? And are all the manifestations of electric charge necessarily associated with mass and its inertial effects? If massless charges exist, then, at the very least, we cannot assume that all energy is derived from matter, just because all matter is necessarily derived from energy or better, itself a form of energy.

Even with respect to the question of the mere addition of mass as a function of increasing speed of a body relative to the inertial frame of the observer's laboratory, it is doubtful whether this should ever be taken to imply an effective increase of the mass of the body in relative translation. For it is at least admitted that in its own inertial frame of reference, the mass of the body would appear to be constant. Yet, this is the only basis that Relativity has at its disposal in order to establish the increase in mass of particles with increasing speed of motion or acceleration, or increasing kinetic energy. Moreover, the reader should note the relation between the length contraction in the direction of relative uniform motion, which implies a contraction of extensity, and thus a compaction of mass in the direction of motion, and the "new kinematic" increase of mass at relativistic speeds. Relativity does not differentiate completely between these two aspects, given that reduction of extension in the direction of motion invariably reduces the volume of a body and thus increases its mass density. When coupled to the increase in mass with increasing relative speed, Relativity is in fact holding two distinct, propositions: that mass is catalyzed from energy with increasing speed, and that its density increases by volumetric contraction with increasing speed. The former depends upon there being no energy loss by radiation, so that effectively it is kinetic energy that is condensed into mass, whereas the latter is a geometric constraint imposed by the Lorentz-FitzGerald transformation.

With SR, the kinetic energy of a body of mass m is no longer given by the Leibniz formula mv^2, modified by Newtonian mechanics into $0.5\ mv^2$, but as

$$E_0/\sqrt{(1-\beta^2)} = 0.5\ mv^2/\sqrt{(1-\beta^2)}$$

where β stands for v/c. Likewise, the inertial mass of a body is not deemed to be a constant, but to vary in accordance with the state of motion of the body and thus with the change in the energy of a body. Hence, the mass-energy of a body is a variable defined by:

$$mc^2/\sqrt{(1-\beta^2)}$$

It follows that if we are to consider the total energy of a body, E_T, it is not simply given by

$$E_T = mc^2 + 0.5\ mv^2$$

but by

$$E_T = mc^2 + 0.5\ mv^2/\sqrt{(1-\beta^2)}$$

But this relativistic increase in mass with increasing acceleration follows from a poor understanding of what exactly is happening in the Kauffmann-Bertozzi experiments: it is massbound charge that, by asserting and conserving its inertia in linear motion, increasingly resists addition of kinetic energy that approaches the magnitude of its rest mass-energy, with the result that what is deemed to become the kinetic energy of the moving massbound charge, and deemed thereby to increase its inert mass, is nothing short of wasted energy thermally radiated from the recalcitrant charge wanting to preserve constant its mass-energy. All happens as if the linear inertia property of the electron and the electrical properties that it entails, are by design inefficient at acquiring kinetic energy, when inertially accelerated. The Argentinian physicist R. Carezanni has poignantly drawn attention to this problematics, and elsewhere we have proposed our own aetherometric analysis of this type of experiments, where it is shown how the experimental velocities of the massbound charges are predicted by a theoretical model that does not take recourse to any of the Lorentz transformations. That means—no time dilation and no relativistic mass increase with acceleration of inertial mass. The inertial mass of a system is only a measure of its rest energy, unlike what SR proposes it is.

These questions surrounding the relativistic treatment of mass-energy with respect to states of motion, and the companion treatment of mass-energy as latent electromagnetic energy, raise the problem of the integration of gravitational and electromagnetic energies and even further, the problem of the energy structure of mass-energy or, in stricter terms still, the problem of what is *the internal motion of a body in a state of rest* with respect to its inertial frame of reference. Specifically, the difficulty resides in the problem of rotation and periodic acceleration, such as these are per force implicated in the actual (intrinsic) motions of material bodies, and most obviously in the ostensibly perpetual motions of

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astrophysical bodies and atomic electrons. These questions raise the spectrum of GR, that is, the problem of how Relativity had to become a generalized theory that encompassed the laws of gravitation, before a unified theory of Special and General Relativity, of electromagnetism and gravitation, could even be sought.

Einstein's first answer to these problems attempted to deal with the problem of rotary motion by considering that "a system of coordinates in stable rotation relative to a system of inertia in the Newtonian sense" yields the identity between gravitational and centrifugal forces proscribed by classical mechanics: the radial force which an observer at rest interprets as a centrifugal force, as an effect of inertia, an observer "immobile" on a rotating system may interpret—by GR's principle—as an effect of a gravitational field. This, of course, was not a legitimate transformation from the viewpoint of Newton's theory of gravitation, since the latter required the field both to vanish at the center of mass and increase radially as a function of the distance, though unaffected by latitude. With GR, Einstein claimed that Space everywhere presupposed a gravitational field—without which, "nothing would remain of Space," not even "a topological Space." This abrogated any independent existence of Space (or Spacetime), but not to the benefit of understanding how multiplicities (that differ in nature and also in metric) are constitutive of energy. Instead, GR's connection of a (spherically flattened) Spacetime continuum to a gravitational aether caved in to mechanistic considerations by making the latter dependent upon matter: "once matter is found in the domain under consideration, only its inert mass [and thus]. . .its energy alone need be considered as actively producing a field." Or, in still more general terms:

> In the generalized theory of relativity, the doctrine of space and time, kinematics is no longer one of the absolute foundations of general physics. The geometrical states of bodies and the rates of clocks depend in the first place on their gravitational fields, which again are produced by the material system concerned.[15]

One may well wonder whether this was the valid route one should take to establish the existence of a non-electromagnetic aether responsible for the gravitational phenomena. For material systems are conceived by GR merely as electromagnetic energy systems that are artificially flattened onto an imaginary topology. Further, this impeded any understanding of mass not just as latent energy, as potential electromagnetic energy, but as *actual* electric energy in a peculiar state of rotary motion. However, at the time of Einstein's first enunciation of the GR theory (1913), there was still an opening left in Relativity with respect to the question of the aether, and specifically, of a massless aether. Certainly SR assumed that the stationary aether was precluded by the results of the Michelson-Morley experiment. But, by the same token, there was no implicit assumption that a *dynamic aether* could not be at work, nor, for that matter, that a non-mechanical *massfree aether* could not exist. Even in this sense, the words of Reichenbach echo those of W. Reich: ". . .Light is an electrical process rather than a mechanical one. . .With such a statement, it is true, the question of the existence of the aether, assumed formerly, is not yet answered in the negative. *All that is proved is that the aether is not a substance, in the mechanical sense of the word, comparable to what we call matter.* The question remains. . . Can't there possibly exist a particularly fine [massless] substance underlying electrical fields and related to them as water is to water waves?"[16]

Most physicists today regard these words of Reichenbach as merely part of the wavering of Relativity in its early days. With considerable confidence, modern physicists assert that waves wave in probability and nothing more. Having become the official logico-mathematical theory of Physics, relativistic orthodoxy, as much as quantum and wave mechanics, refuses to conceive of any form of energy that is not electromagnetic or associated with mass-energy. To speak of the aether these days only brings smiles of contempt from institutional physicists—they have already found something better: the intangible "swarming of virtual particles." Today, the problem of the aether is considered merely one of the great false problems of science and is dismissed outright by slight of hand. Yet, Einstein himself did not consider that his theory of Relativity, in either the Special or the General formulations, precluded the existence of *an* aether. Bergson commented on this essential element of Einstein's Relativity—emphasizing the fact that when Einstein declared that there no longer was an aether, he obviously meant that the hypothesis of a fixed, stationary aether, "a unique and absolutely privileged system of preference," was no longer tenable, *not* that a non-stationary aether *could not, or did not,* exist: for, writes Bergson, "the hypothesis of the aether, suitably amended, may quite well be retaken by the theory of Relativity."[17] In his famous talk on "Aether and Relativity," in 1920, Einstein was clear about the fact that, if space has physical qualities which are not mechanical, then there is an aether which must be compatible with Relativity:

> Most careful reflection teaches us, however, that the special theory of relativity does not compel us to deny the aether. We may assume the existence of an aether; only. . .we must by abstraction take from it the last mechanical characteristic which Lorenz had still left it. . . namely, its immobility. . .To deny the aether is ultimately to assume that empty space has no physical qualities whatever. . .Recapitulating, we may say that according to the general theory of relativity space is endowed with physical qualities; in this sense, therefore, there exists an aether.

It is a disservice to Einstein's theory to present it as if it had, once and for all time, laid the ground for the final dissipation of any "aether illusions." Not only through the back door, the theory of SR as a whole, introduces elements of the old stationary aether theory (the equivalence of all motion, be it rotary or translatory, with inertial motion; the privileged role of the speed of light; the actual extension of Euclidean Space to encompass Time, to name but a few), but

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Einstein is quite explicit with respect to the fundamental physical characteristic of the aether which Relativity can exclude: absolute immobility. After SR, it was the static aether concept which was no longer tenable; not every dynamic concept of an aether had become outlawed. Yet, this was not the conclusion arrived at by twentieth century Physics and the inheritors of Relativity. Einstein's failure to formulate a unified relativistic theory was germane to this posterior switching in the interpretation of Relativity.

One might tend to view Einstein with a somewhat cynical attitude—for, at times he appears to be a traveling road show, gathering theoretical pieces wherever he can—the mass-energy expression from Jeans, the Relativity principle from Mach and Poincaré, the four dimensional Spacetime from Minkovski, the tensors from Riemann, the primacy of electromagnetic energy and its non-mechanical effects from Maxwell, the dimensional transformations from the Lorentz-Fitzgerald contraction, the theory of photons from Planck and the electrodynamics from Lorentz. But the fact was that he alone synthesized all these views, up until then incompatible with one another, into one congealed theory which, nevertheless, *did not forbid a dynamic model of the aether.* Had anyone enunciated such a theory before the MM experiment, one might have realized how the premises of the experiment itself were faulty. From such a perspective, one might well have proposed that there is no aether drift to be expected if the aether around the Earth *moves with the Earth, in the same direction as the Earth's rotation*. In fact, if it moves *with* the Earth it is *likely* that it is *what makes the Earth itself move*. From such a perspective, a null result of the MM experiment would be just as well the predicted or required finding, without any resort to relativistic assumptions. Remarkably, this was precisely the viewpoint that W. Reich would take with respect to the MM experiment: as he suggested, the null result is compatible with the notion that the Earth is surrounded by a rotating (massfree) aether energy envelope that moves from West to East, in harmony with the direction of the Earth's rotation, but slightly faster than the speed of rotation of the planet. The argument has failed to capture the attention of physicists. Yet, it really is not a new idea. Michelson himself had once objected to the relativist interpretation of the null result precisely with the suggestion that his own premise, viz that the aether, if it existed, was stationary, could be wrong: the aether could actually contain moving frames. That was the essential thought behind these explicit words of Michelson:

> [The MM results] could still be accounted for by the assumption that the Earth drags the ether along at nearly its full speed, so that the relative velocity between the ether and the Earth at the surface is zero or very small.[18]

Michelson's point is that, for as long as the motion of the aether, on the surface of the Earth, on the horizon system or the rotating frame, is synchronous or nearly synchronous with the rotary motion of the Earth, we should not expect a phase difference between the light pencils oriented parallel with, and those perpendicular to, the "aether drag." If we can talk of a scientific repressed with respect to the MM experiment, this is its content—that, if we assume that the local aether is not stationary but in motion parallel to the motion of the laboratory locality on the Earth's surface, we cannot conclude from the null result of the MM experiment that the aether does not exist, or cannot exist. Only the classical fixed or stationary aether is ruled out of existence, not a dynamic aether, and specifically not one that may be intimately connected with the rotation itself of the planet.

> *". . .It became clear in the later revision of the [SR] theory that light may be used for all measurements of time, for the designation of the measure of time. . .Clocks and yardsticks . . .have only a subordinate function. They adjust themselves to the geometry of light. . ."*
> H. Reichenbach, *From Copernicus to Einstein,* p. 69, 1942.

4. Relativistic Fragmentation and Spatialization of Time

In our estimation, SR went astray not because it denied the stationary aether, but because, under the influence of Lorentz's tranformations, it negated the physical reality of simultaneity with its concept of Time-dilation and the concommittant inverse correlates of length contraction and relativistic mass increase. Here is where the most intense point of rupture with the romantic and classical notions of Time took place—the rupture with the classical view of a universal Time, a single Duration.

Certainly Relativity compensates for the resulting fragmentation of Time into timelines that can enter only into relativistic quasi-"simultaneity," by integrating these timelines in the Spacetime continuum—by making them into reversible and extra dimensions of Space. But in doing so, all simultaneities are distorted, because the real compensation for the introduction of multiple times is the acceptance of the dilation or transformation of time along any and every line, as a function of relative wavespeed having a uniform limit. The unity of the world is then to be found not in its content, but in its empty and fuzzy form. The reign of the Form in Relativity is also a return to Plato, not Spinoza. It is in this sense that Relativity actually treats the speed of light in vacuum, in Space empty of matter and electromagnetic interactions, as an *absolute* speed, an invariant from which it deduces the pure Form of Spacetime. In the second principle of SR, which serves as postulate for the motion of gravitational waves in GR, we have a *residual notion* of absolute velocity. It is this residue of metaphysics which leads SR to accept that acceleration, irrespective of an inertial frame, has an absolute value of nonzero magnitude. Hence, Sklar wrote:

> If we refuse to adopt a particular inertial frame of reference, the only invariantly characterizable magnitude of acceleration is the zero magnitude. It is [therefore] meaningful to ask, "Is this particle suffering no absolute acceleration at all?" It is not meaningful to ask, "What is the (nonzero) magnitude of the absolute acceleration of this particle?" So we are left [in SR] with a residual part of the notion of absolute acceleration.[19]

Having become the official logico-mathematical theory of Physics, relativistic orthodoxy, as much as quantum and wave mechanics, refuses to conceive of any form of energy that is not electromagnetic or associated with mass-energy. To speak of the aether these days only brings smiles of contempt from institutional physicists—they have already found something better: the intangible "swarming of virtual particles." Today, the problem of the aether is considered merely one of the great false problems of science and is dismissed outright by slight of hand.

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Sklar concludes that "in the special relativistic spacetime . . . there is no such thing as. . .the absolute magnitude of nonzero accelerations," as all other speeds but the speed of light *in vacuo* are relative speeds.

The absence of a universal Time opened the door for a multiplicity of times: everywhere a plurality of speeds for the passage of Time appears to be "affirmed" by SR. Einstein concluded that there was a displacement of every and any simultaneity—that which was simultaneous for a fixed system ceasing to be so for a mobile one—and therefore, he postulated a plurality of times with different speeds of "passage," all real and each characteristic of a particular system of reference, to arrive at his conception of a spatialized unity of Time, this unity consisting in Time simply forming one more dimension of Space. One might say that Relativity experiences the diversity of durations for a multiplicity of observers as a requirement that only partial times can be real:

> In this sense there would be a multiplicity of times, a plurality of times, with different speeds of flow, all real, each peculiar to a system of reference. And as it becomes necessary, in order to situate a point, to indicate its position in time as well as in space, the only unity of time is in a fourth dimension of space.[20]

What Bergson reproached Einstein for was the latter's implicit confusion of the two terms characterizing the Riemannian distinction between actual, quantitative, and discontinuous multiplicities (the domain of the differences in degree) and virtual, qualitative, and continuous multiplicities (the domain of the differences in nature). Einstein proceeds as if there existed only one type of multiplicity, discontinuous multiplicities, condemning himself from the start to never be able to seize the real nature of the continuum. Due to this shortcoming, Einstein will have to invoke notions such as time dilation, the plurality of times of passage and the false unity of a spatialized Time.

Time is not treated by Relativity as distinct in nature or in quality from the set of topological locations or lengths between points. Whether in the SR form of a flat Minkowski Spacetime or in the GR form of a curved Riemannian Spacetime, the essence of Relativity lies in its treatment of Time as the fourth length of a four-dimensional Space. As the actual metric of a length of time is not fixed, only the intervals being invariant, one can no longer speak of the reality of timelines or of synchronicity. Let us consider this fragmentation of Time in some detail.

In Newtonian theory, events take place in absolute Space conceptualized as Euclidean 3D-Space. The set S of spatial locations of an event is given by the three lengths of Space

$$S = x^3$$

whereas the set T of temporal moments of an event, *i.e.* the location of the event in Time, is only the one-dimensional timeline t^1, such that the structure A of Space and Time is simply the Cartesian product of Space and one-dimensional Time:

$$A = S\ T = x^3 * t^1$$

Two events are defined as simultaneous, or are said to belong to the same class if, and only if, they have the same temporal location, *i.e.* occur at the same time, with the result that one differentiates between them solely as a function of the structure x^3. Similarly, diachronicity is simply defined as the reverse procedure whereby events having the same location x^3, may establish diachronic series or collections of events as a function of the structure t^1.

Minkowski Spacetime underlies all relativistic transformations, whether for a special or a general observer. It differs from Newtonian Space and Time in that it is not the Cartesian product $S \times T = x^3 * t^1$, but a Euclidean Space in four dimensions with respect to *topology,* with a non-Euclidean *metric* defined not by the distances between events but by their *intervals*. This is an essential condition for SR, as two observers in motion will in general disagree about the sizes of the spatial and temporal lengths separating two events, but can agree about the interval between events. The pseudo-Euclidean Space[21] is defined as

$$S = x^4$$

but the spatiotemporal structure is given by the interval A along any curve L, defined as

$$A = \int_L dS$$

or, retaining the formalism of length connoted by x and time connoted by t, we have for four dimensional Spacetime

$$dS^2 = dx_1^2 + dx_2^2 + dx_3^2 - c^2\ (dt^2)$$

If we take c as unity, and write the timeline strictly as the length, $t = x_4$, which it corresponds to in Minkowski Spacetime, we have

$$dS^2 = dx_1^2 + dx_2^2 + dx_3^2 - dx_4^2$$

Here, at last, it becomes evident how timeline dx_4 has become reduced to a negative length of four-dimensional Space. Once the metric is chosen, this is still the same notion of an envelope, a container of events separable from the events themselves that was intrinsic to the concept of Euclidean Space, except that it is a four-dimensional pseudo-Euclidean Space. The functional correspondence between a 4D-Space(time) and the Minkowski metric of intervals between paired events, can be functionally written as

$$S = x^4 \text{ -}\int\text{- } dS^2 = dx_1^2 + dx_2^2 + dx_3^2 - dx_4^2$$

As time is turned into just another length, it too becomes subject to a Lorentz transformation, the result being dilation of Time as a function of increasing relative speed. The interval between any two events along any Spacetime curve is a quantity and an invariant property of Spacetime. Unlike distances, which are always non-negative, the intervals of Minkowski Spacetime can be positive, zero or negative, originating different kinds of separations between events. If the interval is positive, a spacelike separation between events takes place; if the interval is negative, a timelike separation

A null result of the MM experiment would be just as well the predicted or required finding, without any resort to relativistic assumptions. Remarkably, this was precisely the viewpoint that W. Reich would take with respect to the MM experiment: as he suggested, the null result is compatible with the notion that the Earth is surrounded by a rotating (massfree) aether energy envelope that moves from West to East, in harmony with the direction of the Earth's rotation, but slightly faster than the speed of rotation of the planet.

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occurs, and if it is zero, a lightlike separation is said to occur. The set of events that have lightlike separation from any given event *a* is denoted as the light-cone of event *a*. Only for events that have a timelike separation can a causal signal be established for propagations *slower* than c. Given the invariant role of c, there can be no causal signal linking spacelike-separated events, which would require propagation velocities *greater* than c.

Minkowski Spacetime is an essentially flat structure that dissociates into Space and Time only when the state of motion of the observer in an inertial frame is specified. That is what gives it its Euclidean or quasi-Euclidean characteristic, by permitting the application of Cartesian coordinates. Hence its employment by SR, where only the special observers in uniform or inertial motion belong to valid frames of reference for the actualization of a system of coordinates. This is what Sklar terms the "preferential coordinatization" characteristic of SR.

Like the flat Minkowski Spacetime of SR, the curved Spacetime of GR is a 4D-manifold with an interval metric and a system of geodesics ("the straightest curved lines"). Just as the Minkowski Spacetime becomes a 4D Euclidean Spacetime once an inertial frame is specified, so does the curved Spacetime of GR become a Riemannian relativistic Spacetime that employs the ordinary Gaussian-Riemannian distance metric but treats not curved n-dimensional Space, but curved 4D-Spacetime, once a general observer is defined by a set of functions. The Minkowski metric of intervals which we wrote above as $S = x^4 -\int- dS^2$, can be treated as an example of a flat n-dimensional Riemannian Space, and with n=4, the metric lengths become replaced by Riemannian interval g-functions, to yield

$$g_{11} = g_{22} = g_{33} = -g_{44} = 1$$

where $-g_{44}$ is now the interval of time between two events, and $g_{ij} = 0$, $i \neq j$.

The relativistic pluralism of times is in the ultimate analysis just a pluralism of intervals, all of which are reduced to lengths of value 0, +1, and -1, such that only 0, x, and -x exist as metric realities. Inevitably, the introduction of negative directions in Time results from the assumed structure of Spacetime, with the result that Spacetime is only irreversible as a process because of the physical limit posed by c.

This relativistic fragmentation of a root-Time into rootlet times, all spatialized and each peculiar to a system of reference, requires a *totalizing unity*, all the more despotic and gratuitous as it empties Space in order to establish itself: it is in the invariant propagation of electromagnetic signals that Relativity finds its fundamental principle of unification, beginning with the reduction of all energy to electromagnetic energy. The unifying element that serves as complement to the dominant reality of "empty space" is the luminal limit to the transmission of energy, which is now exclusively conceived as electromagnetic energy. But this effective dogma-limit of Relativity is a shallow river with a comical effect: four years after Einstein's death, Cerenkov would publish his papers showing the presence in an aqueous medium of *electric* waves traveling faster than the speed of light in the same medium. Much has been made of the fact that Einstein's second principle only applied to the medium of the vacuum. But it would not take long for the founders of QED, Feynman and others, to circumvent the relativistic limit with spatialized fictions of particles actually moving back in time, in their atomicized time. The theory of *anti-matter* finds its rootlets here—from theoretical tachyons that move faster than the speed of light *in vacuo*, to positrons which are taken as electrons moving *backwards in time*. With QED, even irreversibility would be lost from Time. But the unifying Atomistic taboo remained—it is in the Void, in the n-dimensional Space empty of energy because it is empty of matter, that the absolute limit and reference of c lie.

What Relativity, no less than any other theory since Relativity, has been missing is an energetic theory of the continuum, where the manifolds of Space *and* Time would remain mutually irreducible as distinct properties of energy. But to enunciate such a theory, Time itself would have to be understood as having its own universal metric, irreducible to that of Space, and comprising more than one timeline. This requires such a theory to be able to place simultaneity at the heart of the concept of energy, in the domain of resonant synchronization of wave functions. Then maybe one might find that the adequate correlation of dimensions is not additive, but one of superimposition, and thus that the manifolds of Space and Time are themselves directly the product of a constant flux of energy.

> *"It was the advent of modern quantum theory. . .that established the quantum vacuum, so-called empty space, as a very active place, with particles arising and disappearing, a virtual plasma, and fields continuously fluctuating about their zero baselines. The energy associated with such processes is called zero-point energy, reflecting the fact that such activity remains even at absolute zero."*
>
> H. Puthoff, "Can the Vacuum Be Engineered for Spaceflight Applications?" *Infinite Energy*, 3, 15/16, 72.

5. Einstein's and Stern's Proposal of a Zero-Point Energy and Its Legacy

Originally, the theory of the ZPE was a component of Planck's second theory (1909), when Planck actually took discontinuity explicitly into account, albeit still having reduced it to emission processes (absorption of energy from the aether being seen as a continuous and slow process). To understand the ZPE model we must backtrack to the origins of Planck's thought. Unlike Boltzmann's phase-space cell domains, which encompassed a number of molecules, Planck's original search targeted the units of a continuum, his theorized phase-space cells accordingly having to be *quite small domains of space*. Planck proposed that these phase-space cells are electromagnetic oscillators (resonators) which may possess all possible energy levels of the spectrum. The physical cells of space would contain discrete elements describing in two

In our estimation, SR went astray not because it denied the stationary aether, but because, under the influence of Lorentz's tranformations, it negated the physical reality of simultaneity with its concept of Time-dilation and the concommittant inverse correlates of length contraction and relativistic mass increase. Here is where the most intense point of rupture with the romantic and classical notions of Time took place—the rupture with the classical view of a universal Time, a single Duration.

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dimensions cyclical ellipsoids such that, once an oscillator crossed the nth elliptical boundary in the phase plane, it would radiate n quanta by emission, and would finish radiating once the ground state of zero energy was reached. Even at absolute zero temperature, the oscillators would possess finite energies randomly distributed between zero and $h\upsilon$. This, however, only becomes clear with his second theory. There, he added another element: the presence of a residual randomistic energy near absolute zero temperature which suggested to Planck that the energy of an oscillator, set in a weak, fluctuating stochastic "field," could easily be brought to the point of electron emission and converted into the translatory kinetic energy of an emitted electron. This had direct application to the understanding of the photoelectric effect and formed the basis for Einstein's demonstration that the electromagnetic energy of photons would become transformed into the kinetic energy of the emitted electrons. But Planck's second theory postulated that only the number of electrons emitted could vary, *not* their energy, suggesting therefore that, if Einstein's law of the photoelectric effect was correct, there was no need to assume that these electrons were emitted from the light-particles themselves.

According to Planck's second theory, the submicroscopic oscillators were described with an additional term, $h\upsilon/2$, to denote the energy present when all thermal oscillation had ceased, but which was absent from the distribution law for radiant energy (being negligible for $h\upsilon \ll kT$) though it would be accounted for, in principle, by finding that the frequency of all motions contributing to the specific heat of a gas was independent of temperature.

Paradoxically, it was Einstein and Stern in 1913 who set the ZPE theory on an independent course, when they demonstrated that while this foundation for the additional term might well be appropriate for Planck oscillators, and even possibly for the vibrations of atoms in solid lattices, *it could not be applied to the rotational frequency of a diatomic gas molecule* (which Einstein and Stern treated without quantizing rotational energy). Based on then recent measurements of the specific heat of hydrogen at low temperatures, they concluded that existence of zero-point energy on the order of $0.5h\upsilon$ was most likely. In the second part of their paper, however, they provided a derivation of Planck's Law without taking recourse to discontinuity, by assuming that the value of the ZPE was simply $h\upsilon$. It is worth noting that Einstein had already in 1905[22] framed the problem of discontinuity, even if only heuristically, as one of placing limits upon the infinite energy of the vacuum state predicted by the Rayeigh-Jeans dispersion law. According to Einstein, the Rayleigh-Jeans law would result in an impossibility, the existence of infinite energy in the radiation field, and this was precisely incompatible with Planck's discovery—which suggested, instead, that at high frequencies the entropy of waves was replaced by an entropy of particles. Einstein, therefore, could only hope for a stochastic validation of Maxwell's equations at high frequencies "by supposing that electromagnetic theory yields correct time-average values of field quantities," and went on to assert that the vibration-energy of high frequency resonators is exclusively discontinuous (integral multiples of $h\upsilon$).

Since then, the ZPE model evolved as an experimental physical theory independent of Planck's second theory, the latter having faded away, largely because of the Bohr theory which postulated identical discontinuous processes at work *in both absorption and emission of energy*. Confronted with the Stern-Gerlach experiment of 1922, Planck too would accept the stationary states and the quantization of energy proposed by the Bohr theory. Hence, for modern physics, the lowest energy of any harmonic oscillator, or any particle in a given parabolic trajectory, cannot have less than this zero-point energy, defined for quantum mechanics as $E_0 = h\upsilon/2 = h\omega_0/4\pi$, where ω_0 is the angular frequency of the quantum harmonic oscillator which, unlike the classical harmonic oscillator, is not independent of the amplitude of the oscillation, and thus is not independent of energy. The ZPE was treated as a completely nonclassical phenomenon and led, in a very real sense, to the creation of quantum mechanics.

That this definition of a "new aether" was in Einstein's mind is proven not only by his conclusions from SR and GR, and his joint proposal of the ZPE of space, but also by the explicit proposal that he made in 1925, when commenting on (and commending) de Broglie's theory of matter waves, for a universal electromagnetic limit frequency of this "motile" or nonstationary aether, to be determined as:

$$\upsilon_\delta = m_e c^2 / h = 1.2353 * 10^{20} \text{ cps}$$

This would have the result of giving E_0 as half the mass-energy of the electron

$$E_0 = h\upsilon_\delta/2 = m_e c^2/2$$

Einstein's proposal, similar to others made by Dirac and Schrödinger, would later become one of the cornerstones of Aspden's theory of aether spin. These facts demonstrate the length to which Einstein went, up until 1925, in admitting a compatibility between Relativity and a theory of a minimum continuous moving field responsible for electromagnetic quantization and for gravitation.

In more recent decades, different approaches to ZPE theory have ignored these elements of Einstein's work and have gone on to characterize the "new aether" as a "free field" (the ZPF) of electromagnetic radiation present in space devoid of matter. Since the 1970s, stochastic electrodynamics (SED) has developed as an alternative interpretation of quantum mechanics, designed to treat quantum field-particle interactions in accordance with classical physics. SED theories retained the probabilistic approach that characterized Planck's and Einstein's early efforts by positing that the "vacuum state" is actually a field formed by a collection of classical electromagnetic radiation modes with random phases, present even at absolute zero temperature, the ZPF being homogeneous, isotropic, and subject to Lorentz invariance.[23] A real electromagnetic field is supposed to define the "vacuum" state as a background of classical radiation modes, while ZPF-induced motion involves a free energy exchange between the field and the particle with no average transfer of energy in any direction at any frequency.[24] From this vantage point, heat has two forms: (1) as thermal energy, which is but a form of electromagnetic radiation that flows from the regions of space surrounding the system undergoing a temperature increase, *in a reversible process,* such as displacements driven by van der Waal or Casimir forces (if temperature $T \neq 0$); and (2) as emitted radiation from the system to the ZPF, resulting from conversion of the kinetic energy of two colliding particles or molecules, *in an irreversible process* obeying a positive net entropy change.[25]

It is fascinating to see the unresolved problems of con-

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temporary physics dredging up the sediments of forgotten disputes, returning incessantly to their grounds. If there is energy in space even at absolute zero, this energy flux, this motion, cannot be due to thermal energy, to sensible thermal energy, but the cause of it. And if this motion exists even for space devoid of matter, then the question becomes—what moves if it is not matter? What energy is it? If what moves are *actual waves of energy* that account for the translational and rotational energies of atoms and molecules at low temperatures, the primary motion of these waves must clearly be distinct from the secondary motion of the molecules it gives rise to, the Brownian motion of the latter being a particular consequence of the "random phase" motion of the former. These questions are distinctly reminiscent of the problems associated with determining the nature of Planck's oscillators. Just what exactly were they? Were they elements of matter or elements of the (imponderable but stationary) aether? Could they have been the elements of an imponderable and moving aether? Certainly their behavior was particulate and, for as long as we relate particulate nature to mass, the difference between matter and the aether is not very significant, as the aether in a classic sense was considered to be a material substrate (with, as we have argued, Maxwell's non-material, non-mechanical "field aether" being the partial exception to this statement). Because this difference indeed has not been properly considered by modern physics, quantum electrodynamics (QED) has suggested that space is filled with virtual particles fleeting constantly in and out of existence—a domain that has become the impregnable realm of imaginary matter and anti-matter couples that fill up the quota of "dark matter" which is missing from the relativistically-revised electromagnetic Weltanschauung. In fact, it is here that SED deviates from QED to posit instead a random phase of electromagnetic wave continua. The question then is—just what are these space elements, nodes of electromagnetic waves or virtual particles, or neither?

Already at the dawn of Planck's first theory, Ehrenfest had proposed a duality of oscillators, molecular resonators, and aether vibrators. Resonators were said to be the ultimate elements of matter and were conceptualized as being interconnected by a network of springs or vibrators representing the aether in different states of oscillation. Even though Ehrenfest's early interpretation of the need to introduce discontinuity—to reach Planck's goal of determining a unique distribution function—suggested that non-colliding molecular motion was a form of energy transmission which betrayed modes of free aether vibration in the absence of increase in entropy, it was Einstein's statistical approach to the thermodynamics of mechanical systems which would triumph on the very basis that the fixed size h should not be attributed to phase-space cells, *but only to the probabilistic behavior of every physical system as it inevitably tends over time towards a more probable stationary state*. Planck's hope for a theory that would keep constant the size of phase-space cells fell by the wayside, as it violated the very probability ruling the transformations. But, as we have seen above from Reichenbach's words, Special Relativity also introduced a new element when it drew the conclusion that the stationary aether of the wave-theory of light could not, and did not, exist. If the aether existed at all, it could not be a material medium; its physical nature would demand precisely that it be massless and non-mechanical. Yet, Special Relativity had made the aether vibrators irrelevant, even as it decoded the nature of the aether one step further.

The only connection that mattered for Einstein at this conjuncture was the relation of any system to that "infinite heat bath" with which it is in constant communication. Indeed, having arrived at this very point, Einstein focused on the phenomenon of energy fluctuations, to posit that the only system where energy fluctuations are presumed to occur is "empty space filled with thermal radiation." And this then throws us right back to the very same problem: if it is not thermal energy resulting from the radiative transitions of decaying high-energy matter, as Einstein would later hold, just what is the nature of this radiation? Current SED theorists propose that its nature is electromagnetic, and that the ZPF is Lorentz invariant. As Aspden has quite appropriately commented, this "fits too closely in the Einstein mould and accounts for his failed attempts to explain inertia."[26]

> *"After such bad experiences, this is the moment to forget the ether completely and to try never to mention its name. We shall say: our space has the physical property of transmitting waves, and so omit the use of a word we have decided to avoid."*
> A. Einstein and L. Infeld, *The Evolution of Physics,* 1938, p. 176.

6. Conclusion: How SR Voided a Physics of Energy

One could suppose Relativity to be the only quasi-comprehensive theory of Physics that first put forth an energetic argument. Yet, energy only figures in Relativity as a manifestation secondary to the structure of the Spacetime manifold, and subject to relativistic constraints, by its reduction either to electromagnetic or mass-energy. If Relativity were to be an energetic theory of Physics, it would have to abandon the very canon of the 4D-Spacetime manifold, and realize that the dimensionality of any continuum would be the dimensionality of massfree energy. As it stands, Relativity is not an energetic theory but a formalistic or geometric theory of nature, where energy is dissociated from the structure of Space to the exclusion of Time.

Several steps are required to form the relativistic argument that finally *severs* Space and Time *from* energy and motion, to make of the block Space-Time a mere geometric construct. They are:

a) Time is a property of space as energy is a property of matter.

b) Space empty of matter is empty of energy, as it is empty of matter waves (even the photon must have weight, like the atom, because of a postulated inertial mass).

c) Space empty of mass-energy is space empty of motion: kinetic energy only exists as energy associated with a body, energy of the motion of a body.

It is fascinating to see the unresolved problems of contemporary physics dredging up the sediments of forgotten disputes, returning incessantly to their grounds. If there is energy in space even at absolute zero, this energy flux, this motion, cannot be due to thermal energy, to sensible thermal energy, but the cause of it. And if this motion exists even for space devoid of matter, then the question becomes—what moves if it is not matter? What energy is it?

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d) Space exists therefore independently from matter, energy and motion: it is an empty geometric form, and it is this form that explains gravity. Space is the scheme of matter and renamed Spacetime.

Only the mathematical and geometric evolution of form determines the function and the codistortion of Space and Time. It is Space which is the form of both matter and energy, their scheme. As Spacetime it is Space that exists as pure form, at the cost of eliminating a dynamic and economic aether—and at the cost of eliminating singularities:

> A field theory is not yet completely determined by the system of field equations. Should one admit the appearance of singularities?. . .It is my opinion that singularities must be excluded.[27]

At the end of his life, Einstein was still searching for a theory of the continuum that would eliminate singularities—caught in the same problem that prevented de Broglie from succeeding in integrating wave and quantum mechanics in his wavicle theory of matter waves. And why? The simplest answer is—because of the classical electromagnetic ignorance of longitudinal electric waves. In fact, the adoption of Maxwell's theory of transverse, homogenous electromagnetic disturbances to establish a bridge between quantum and wave mechanics prevented any synthesis with the behaviour of quanta. De Broglie summarizes the difficulty:

> In the theory of waves, the magnitudes of fields which are propagated by undulations are distributed in space in a continuous manner; the different points of the wave do not differ essentially one from the other, which can be interpreted in the language of mathematics by saying that the waves of the usual undulatory theory do not present any "singular" point.[28]

Several barriers prevent any filiform treatment of wave functions by current electromagnetic theory: the conceptualization of the propagation wave as a constant transverse wave which precludes singularity; the notion that the wave function can only achieve a filiform solution if its frequency is infinite; and the notion leftover from Einstein, Podolski, and Rosen, that nonlocality can only invoke "spooklike actions at a distance." To us, it is apparent that not all hidden variables are local, but just as well that nonlocal variables are only relevant if they affect local ones (relativity of nonlocality). Quantum mechanics has remained to this day incomplete, and this is the source of the paradox. It would suffice to explain how electromagnetic quanta are locally generated with distributions dependent upon the motion of continuous, nonelectromagnetic, longitudinal waves, for the seemingly impossible integration of quanta with an energy continuum to reveal itself as a shallow problem. Instead, modern Physics is stuck with the quasi-filiform relativistic solutions of Sommerfeld and Schrödinger, trying to insert a singularity into a probabilistic wave that approaches the Maxwellian envelope.

Furthermore, experimental findings of nonlocality employing anomalous dispersion techniques that exploit stimulated emission[29] are easily explained by precisely the assumption that the stimulus is propagated supraluminally because it is not an electromagnetic stimulus to begin with. The NEC researchers have wrongly contended that "time and the group velocity being negative," the photon pulse arrives before it departs (a nonsense even for relativists), but in fact all that the nonlocality response demonstrates is that the shortened, though positive, time of propagation is not due to electromagnetic factors. In fact, the symmetry of choices made by synchronized photons in Franson-type experiments should be read as proof that microscopic simultaneity does exist. What the researchers interpret as "photon choices" is in fact an implicit admission of a synchronization phenomenon. If the light stimulus can propagate at speeds greater than those of light, as Cerenkov first found out, then what propagates is not light at all, and thus certainly not energy configured electromagnetically. Light consists solely of a local production of photons in response to the propagation of a stimulus that itself is not electromagnetic. The aether is not about words (nor about blocking out the infantile trauma of "bad experiences". . .), but about a Physics of massfree energy and Time, that is, about simultaneity and the paradoxes of synchronicity. Only deluded fools believe one can mathematically omit *that* by playing with a metric of intervals, simply because the notion of the aether as a static mechanical medium proved to be false.

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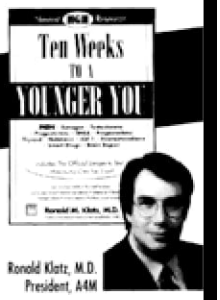

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ECLECTIC OBSERVER

Compiled by Eugene Mallove

Because this issue of *Infinite Energy* is devoted to reconsidering Einstein's work, this column has compiled an eclectic gathering of "Einsteinia" to accompany a few other matters.

Abstracts from *Physics Essays*

The following two abstracts are from papers by Professor Emeritus of Physics, University of Connecticut, Howard C. Hayden, the former editor of *Galilean Electrodynamics* and the present editor of the hard-hitting newsletter, *The Energy Advocate*. (PO Box 7595, Pueblo West, CO 81007, www.EnergyAdvocate.com) Readers of other fine Einstein critiques that appear in this issue of *Infinite Energy* may wish to consult these particular works by Professor Hayden:

Abstract: (*Physics Essays*, Vol. 4, No. 3, 1991, pp. 361-367)

Is the Velocity of Light Isotropic in the Frame of the Rotating Earth?

Einstein's second postulate refers only indirectly, through the first postulate, to inertial reference frames. Still, few textbooks proclaim that the speed of light is constant in all reference frames. Since the rotating Earth is not an inertial system, it is reasonable to ask what the speed of light is with respect to a laboratory reference frame. The results obtained from several published experiments are fully consistent with vector addition of light velocity with velocity due to Earth's rotation.

Abstract: (*Physics Essays*, Vol. 8, No. 3, 1995, pp. 366-375)

Special Relativity: Problems and Alternatives

Despite a near century of impressive successes and nearly universal acceptance by physicists, Einstein's special relativity theory (SRT) has several problems, most of which arise from a lack of experimental confirmation, but some of which seem to imply a direct contradiction between experiment and prediction. First and foremost, SRT is a theory of relativity, neatly codified in the Lorentz transformation equations (LTEs), which provide a prescription for converting coordinate values in one inertial system into those of another. That a direct comparison of measurements in two relatively moving reference frames has never been done shows that at least half of the theory is unconfirmed. Also, the much-heralded Lorentz contraction has never been demonstrated experimentally. Absence of evidence is not evidence of absence; neither is it evidence of presence. In SRT, when the LTEs are applied to two systems A and B moving at right angles to one another (as seen from a third reference frame O), the velocity of A with respect to B is not the negative of the velocity of B with respect to A. SRT then involves "Thomas rotation" to explain the asymmetry, although nothing actually rotates. But there is also abundant evidence to show that SRT must, at the very least, engage in a tortuous reasoning to explain some experimental results, among them stellar aberration (which in SRT depends upon relative velocity of Earth and star); the Sagnac and Michelson-Gale experiments; the Allen around-the-world Sagnac experiment; the Hafele-Keating experiment; the Brillet-Hall experiment; and the Champeney-Moon experiment.

Abstract from *Galilean Electrodynamics*

Cynthia Kolb Whitney, a Ph.D. physics graduate of MIT and the present editor of *Galilean Electrodynamics* (she has been in that role since early 1997), published a series of technical articles in that journal in 1996 and 1997. They are titled: "Finding Absolution for Special Relativity Theory—Parts I, II, and III." [7, 1996, pp.23-29; 7, 1996, pp.63-69, and 8, pp.9-15]. The abstract to Part III is revealing:

"In Parts I and II of this series, Einstein's special theory of relativity was identified as just one member from a large family of coordinate transformation theories that differ only in the details of a single parameter pertaining to clock desynchronization. A *different* family member was shown to be required by the well-known Sagnac experiment. The present paper shows how the corresponding single parameter of the new theory can also be obtained from a postulational basis different from that of SRT, but nevertheless perfectly plausible. This is important because we now have both an experimental basis and a theoretical basis for a new coordinate-transformation theory that avoids some of the criticisms often leveled at SRT. For example, the present theory does not have a twin paradox."

We quote her very important and dramatic conclusions, which appear at the end of her Part III article:

"One can never say that science actually comes to conclusions, since everything is provisional and subject to later revision. But the present work does at least support a few bold 'conclusions.' They are:

1) Light propagation is a two-step process. First there is expansion from a source, then there is collapse to a receiver. Both processes transpire at speed $2c$. Just about everything we know about classical physics follows from this two-step propagation model.

2) The Lienard-Wiechert models were wrong. Having been based on one-step propagation at c and not on two-step propagation at $2c$, they could not be right. But they could be rationalized within the context of SRT, and so they stuck with us for a century.

3) Einstein was wrong. Einstein used the one-step propagation model with speed c. All of the peculiar effects attributed to SRT follow from this model. That is, Lorentz contraction of lengths, apparent slowing of clocks, and

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material speed limitation are all optical illusions created by imposing the one-step model on a two-step process.

4) Einstein was wrong again. He formulated his theory in a way that limited the speeds of coordinate frames to the speed of light. Coordinate frames should be allowed unlimited speeds, because coordinate frames are mental constructs, not physical things.

5) Reality is almost Galilean. Clearing away the optical illusions almost leaves just old fashioned Galilean space-time; it differs only in supporting light propagation at finite speed. But rods do not really shrink and clocks do not really slow down. The famous paradoxes of SRT disappear. For example, the purported differential aging between separated twins becomes a non-problem.

6) There are big consequences. Physical objects actually move with Galilean speed *V*, which is unlimited, not with Einsteinian speed *v*, which is limited to light speed c. In the present theory, *v* is simply an observed velocity, subject to an observation error caused by application of a one-step model for light propagation. Many mysteries are thereby cleared up. For example, astronomical observation provides some evidence of superluminal speeds in cosmic jets. This is not in fact impossible. The significance of the observation error here identified depends on orientation, and becomes nil when the observed motion is transverse to the observer. Also, many conceptual doors are opened. For example, long-distance space travel is seen to be not impossible in principle. We are now limited not by the speed of light, but rather by the speed of thought, which the present author submits is actually infinite when thought is liberated from dogma."

Memorable *Discover* Cover

The January 2000 cover of *Discover* magazine features a photograph of Albert Einstein in the middle of a hearty laugh. Bold headline on the cover: "The Most Amazing Century of Science." Then a tiny caption near Einstein's cheek: "Albert Einstein gets the last laugh: His theories stand the test of time." There is no lengthy feature article about Einstein, but he is part of a century-long timeline. In the period 1900-1919 *Discover* notes that Einstein "threw science a curve. . .his special theory of relativity overturned the Newtonian view of space and time."

"Gaping Hole in Mathematics" Relevant to Relativity?

This item founds its way into my electronic in-basket. It may have some relevance to the relationship of mathematics to physical theory, which is of course a major issue in trying to decide what kind of theory of relativity—Einstein's or someone else's—is valid. From *Netsurfer Digest,* Vol. 7, #8, March 27, 2001:

> Huge Gaping Holes in the Foundations of Mathematics. One of the profound questions about mathematics—and in some sense about reality— is whether we discover it or make it up. Do mathematical theorems exist in the ether as eternal truths waiting for us to stumble upon them or do we invent them in our fevered imaginations.
>
> Mathematician Gregory Chaitin has done some thinking about this, cleverly standing on the shoulders of giants to give him the panoramic view. He has proven that an infinite number of mathematical facts exist—seemingly resolving the question. But he also proved that these mathematical facts are entirely random, totally unrelated to one other. If mathematicians find any relationships, they do so by luck - "math is true by accident." Now, here's the kicker. Gregory's discovery basically means that any theory of reality - the holy grail of physics, the Theory of Everything— will never be mathematically consistent. These two links tell the story of Gregory's uncomputable number, Omega, how it blows huge holes in the whole of mathematics, and how it places fundamental limits on what can be known about reality. Think about that next time you cross the street.

Gregory: http://www.cs.umaine.edu/~chaitin/cmu.html

Story at this link:
http://www.newscientist.com/features/features.jsp?id=ns22811

Cold Fusion in New Sci-Fi Book

Subscriber Mitchell Jones noticed a reference to cold fusion in the science fiction novel by L. Neil Smith, *Forge of the Elders*. He posted this excerpt on the Vortex discussion group:

> "Well, leaving nine-tenths of Earth's gravity behind represents considerable relief in itself. It also serves to keep things—bottles, glasses, one's skeletal calcium—in their places. I'm grateful to our Bureau of Suppressed Technologies that, instead of the better part of a year, the voyage will last only days. To think that America might have had cold fusion decades ago!"
>
> "It gored too many well-fed oxen," Mubakkir agreed, "petrol cartels and power collectives, so they buried it and discredited its discoverers."
>
> "So much for free enterprise!" The Banker laughed again.

❑ ❑ ❑

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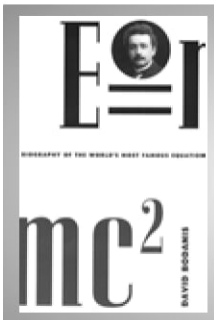
Book Reviews



E=mc2: A Biography of the World's Most Famous Equation

by David Bodanis
ISBN 0-8027-1352-1
$25.00 Hardcover, 337 pp.
Walker & Co., 2000

Review by Bill Cantrell

This book is a little different from most of the "recreational reading" hardbacks about Einstein and his science. The author's stated goal is to explain the famous equation, $E = mc^2$, to the *layperson*. Hence, Bodanis has his work cut out for him. The book is something of a mix between soap opera, suspense novel, and a *Boy's First Book of Science*. Overall, he does a good job of explaining a technical subject in an interesting way to the uninitiated. And there are portions of the book that will appeal to technically oriented people with an interest in the history of science.

For the scientific reader, a little recalibration is required to adapt to the non-technical style of presentation. Certain desirable facts and details are missing. For instance, the book devotes an entire chapter to Michael Faraday's life, but mentions only his invention of the electromagnetic motor in 1821. (The first electro*static* motors were invented in the mid-eighteenth century. Faraday's greatest achievement was electromagnetic induction in 1831.) But a lot of important details are left fuzzy so as not to slow the book's pace for the layperson. Bodanis compensates for this by enlarging the Appendix in the back to include excerpts from private correspondence and other references.

The reader is given a glimpse into the personal lives of some of the early scientists tied to the famous equation. Some of the notables include du Châtelet, Curie, Lavoiser, Maxwell, Oppenheimer, and Rutherford. The book gets off to a slow start, but quickly picks up the pace. There is a delightful chapter on the first accurate calculation of the speed of light in 1676, using the eclipse of Jupiter's moon, Io. The story is all the more interesting because of the public relations war being waged between the young maverick Roemer and the great authority Cassini. Despite Roemer's correct analysis and prediction, the experts continued to maintain that the speed of light was infinite, just as they had always thought.

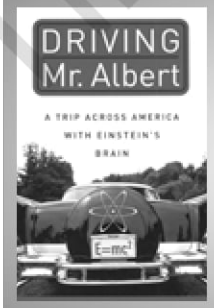
However, if you are looking for information about the personal life of Albert Einstein, you won't find it here. Apparently, Einstein's private life was not very interesting, and virtually no mention is made of some of the less than flattering events. For example, the book devotes one sentence to the fact that Einstein had an illegitimate daughter with his wife to be, and the child was given away before they married. Buried in the Appendix, we learn that Einstein had an affair leading to divorce, and that later, he went through a second divorce.

In a chapter on nuclear fusion, we learn of the fight against sexism and discrimination that Cecilia Payne had to endure in her career as an astrophysicist. She was the first to recognize that the Sun was composed mostly of hydrogen, *not iron*, as originally thought as late as the 1920s. When she suggested that nuclear fusion was occurring, the establishment did its utmost to punish and discredit her.

The race to achieve nuclear fission is covered in detail, both in Germany and in America. There is a chapter devoted to the sabotage of a Nazi-operated heavy-water manufacturing facility in Vemork, Norway. The author recounts the nail-biting exploits of British glider teams and Norwegian Resistance fighters.

With 20-20 hindsight, the book pays homage to revisionist history by devoting a few pages to the belief that the use of the atom bomb was not justified in winning the war with Japan. (It is *conservatively* estimated that one hundred thousand American and three hundred thousand Japanese lives were saved by using the atomic bomb.) A chapter is devoted to some of the technical details of the arming mechanisms for the Hiroshima bomb—the clock timer, the barometric timer, and the radar-like altitude sensor. Here we get a fascinating ringside seat, all told in slow motion, of what the fissionable material was doing, how fast it was propelled into a developing fireball, the temperatures involved, and the radiation unleashed.

Overall, the book covers a broad and interesting range of material. It is easy reading, and can be completed in a few leisurely evenings.



Driving Mr. Albert: A Trip Across America with Einstein's Brain

by Michael Paterniti
ISBN 0-385-33300-5
$18.95 Hardcover, 211 pp.
The Dial Press, 2000

Review by Christy L. Frazier

It is difficult to recommend a book to an audience when not sure if the book is worth its time (fortunately, at 211 pages, the book is not dense in either ideas or length and can be read quite quickly). If you are looking for insight into the life and work of Einstein, then this book is not for you. But, if you are up for a quirky romp through eleven days of cross-country travel with two equally odd characters (and Einstein's brain cut up into many pieces, placed in formaldehyde-filled Tupperware and stashed in a duffel bag), then you might enjoy *Driving Mr. Albert*.

This is the story of author Michael Paterniti's encounter with Dr. Thomas Stoltz Harvey, the pathologist who autopsied Albert Einstein's body after his death on April 18, 1955. Depending on one's point of view, he is the man who either "saved" or "stole" Einstein's brain. That issue is at the heart of the story, but Paterniti never completes a full-circle critique of Harvey's actions. His compassion for the man who was ousted from the Princeton community for his actions and afterward lost (or gave up) his career and became a factory worker, is not tempered by equal criticism of the

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pathologist who stepped outside the bounds of medical ethics and apparently went against the wishes of Einstein (who, in his dying days, urged his family members not to let his home turn into a museum and, presumably, did not want his body cut up for science) and his family. Einstein's estranged sons and ex-wives learned of the removal of his brain only from the front page of *The New York Times*.

Driving Mr. Albert chronicles the strange cross-country journey of these two men, who are bound for California so that Dr. Harvey can meet with a neuroanatomist who is studying the brain. They also will meet Evelyn Einstein, Albert's granddaughter (perhaps for Dr. Harvey to assuage some new-found guilt for his actions?). Since the autopsy in 1955, Dr. Harvey kept most of Einstein's brain in his home; it was seen by very few people. Small pieces were sent to a few researchers, but not until many years later, calling into question Dr. Harvey's assertion that he removed the brain for study. One study, published in 1999 by McMaster University (Ontario, Canada), concludes that Einstein's brain was of slightly less than average size for a man of his age, but that the part of the brain that controls mathematics, music, and visual images was 15% wider than average. This study has been widely criticized as inconclusive as to Einstein's "genius," because no comparison has yet been made between his brain and that of other physicists/mathematicians, living or dead. The biggest problem with any study of Einstein's brain is that it was not preserved properly when removed by Dr. Harvey.

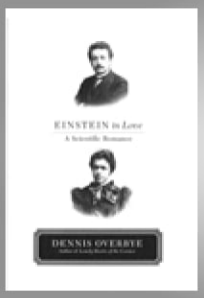
The stops to visit relatives and tourist attractions, while odd in their own right, are not the most interesting aspect of this story. What is utterly intriguing and perplexing is that Paterniti agrees to drive Dr. Harvey three thousand miles without first demanding to see the famed brain. Across this great land, Paterniti attempts to see the brain many times and is baffled that it is generously shown to strangers but not to him. Since Dr. Harvey identifies Paterniti as "the chauffeur" and barely engages him in any meaningful conversation, one wonders throughout if Paterniti will ever see the brain.

There are some books that seem perfect for becoming a movie screenplay. Though I am not usually in favor of books being turned into movies, *Driving Mr. Albert* has "Hollywood road trip flick" written all over it. This could be immortalized on film as a cross between "Thelma and Louise" (no crime has been committed, but the fact that a brain is in the trunk could easily lead to comedic run-ins with the law) and "Rainman" (the "good" doctor has some rather odd personality traits which lead to some rather strange encounters). A documentary, "Einstein's Brain," was made in 1994, but a moviemaker would have poetic license to stretch the story to a level of absurdity and humor that the book and documentary cannot, but maybe should given the subject matter.

Truth will ultimately prevail where there is pains taken to bring it to light.

—George Washington

Submitted by Walter S. Rosenthal



Einstein in Love: A Scientific Romance

by Dennis Overbye
ISBN 0-670-89430-3
$27.95 Hardcover, 416 pp.
Viking Press, 2000

Review by Soo Seddon

I'm at a loss to work out why Dennis Overbye gave his book such a cutesy and not entirely appropriate title.

Einstein in Love is basically a biography of the great man from the days when he wasn't so great in the eyes of the Zurich University professors, whose classes he frequently skipped, to his international renown as the shining prophet who introduced the world to relativity theory.

The book's charm lies in the intriguing revelations uncovered by the author's exhaustive search through Einstein's recently uncovered personal correspondence. It covers the period from 1896 to 1919, spanning the interval between Albert's student days and Eddington's announcement of his confirmation of the light-bending effects postulated by the theory of General Relativity.

For a man whose brain had a parietal lobe 15% larger than normal, Albert Einstein never really mastered the spatial relativity between himself and members of the feminine gender. As Overbye states, "Albert was always trouble for women."

The young Albert rapidly distanced himself from his first love, Marie Winteler, when he discovered the delights of physics. She was far too fluffy and not the substantial sort of wife a serious scientist should become shackled to.

At University in Zurich he met Mileva Maric—a very substantial and sombre fellow student from Serbia; she was the only woman in his physics class and Albert soon found himself attracted to her fine mind and sober intelligence. He continued, however, to correspond with the abandoned Marie, who still happily did the laundry he posted to her regularly.

Albert's parents hated Mileva and considered her totally unsuitable as a life companion for their son, a fact which placed disastrous stresses on the relationship.

When Mileva suspected she was pregnant, Albert wrote to her that she shouldn't worry as she was his "own dear sweetheart," adding in the next sentence, "I am not at all satisfied with the way my theory of thermoelectricity is going."

Mileva gave birth to a daughter, Lieserl, whom Albert never saw. She was placed in the care of Mileva's relatives and there is no further mention of her in correspondence. It is not known if she was adopted or died. This "secret" daughter did not become known to the public until 1987, when the first volume of Einstein's collected papers was published.

Albert was working at the Swiss Patent Office when his father, on his deathbed, gave permission for Albert to marry Mileva. Albert wasn't at all that sure about things, but did what he later called his "duty" and the newlyweds went to live in Bern.

In time, two further children were born to them, both sons.

The issue of whether Mileva ever actually helped Albert

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with his work is raised several times, but mainly by reference to a biography of Mileva which claimed it was actually she who was responsible for the relativity theory. This idea is scoffed at by all of Einstein's biographers. Mileva failed to gain her degree despite several attempts and eventually abandoned science as a career.

In a letter to relatives following the finalization of the Special Relativity theory, Mileva wrote: "We have just finished some work that will make my husband world famous."

Their marriage soon began to falter. At the "Witches Sabbath" (Albert's name for the First Solvay Congress), he told a colleague that marriage was "an attempt to make something lasting out of an accident."

Following publication of his theories, Albert was being head-hunted across Europe and tended to play educational establishments against each other, usually being offered more money to stay than he would acquire by leaving.

On a trip to Berlin to check out yet another lucrative professorship offer, he was reunited with his divorced cousin Elsa, to whom he was very quickly romantically drawn. No correspondence from Elsa survives; she requested that Albert burn her letters immediately after reading them. Mileva was a very jealous woman, so Albert complied.

The dalliance with Elsa lasted a month, then Albert stopped the relationship, only for it to be rekindled a year later when Elsa wrote him a birthday letter.

Einstein never actively pursued women; he was always passive and relied upon them pursuing him—which they invariably did. He also never blamed himself for anything. When Elsa, then established as his mistress, suggested divorce from Mileva, Albert said that would be difficult as he had no proof of his wife's infidelity!

Eventually Albert and Mileva divorced and a part of her settlement was his promise to give her the money from the Nobel Prize he hoped eventually to receive. Although it sounds presumptuous, Albert by then had been unsuccessfully nominated for the Nobel Prize seven times and it was surely only a matter of time.

Now free to marry, Albert dithered because he believed himself to be in love with Elsa's daughter Ilse. He informed both women of his dilemma and stated that he could marry either one of them, so he would leave it to them to decide between themselves.

An extraordinarily tolerant Elsa passed the choice to her daughter, believing that possibly she had feelings for Albert and that he would prefer a younger wife in order to have children. Bizarrely, they had no idea that Albert had two sons and a lost daughter already. He appeared to have successfully kept his two lives very separate.

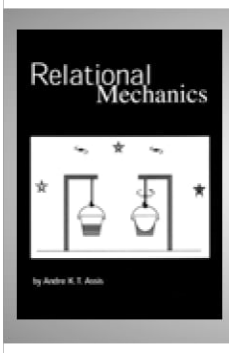
Ilse declined his "proposal" and Albert and Elsa were eventually married. A footnote mentions that Albert remained married to Elsa until her death in 1936 although he had many extra marital affairs—some of them with Elsa's permission!

This fascinating book left me with the feeling that, possibly, emotional dysfunction and genius must of necessity go hand in hand. Clearly Einstein was distracted for most of his life and resented the intrusion of human affairs into his plans.

He wrote that Mileva frequently deliberately created unrest for him at crucial points in his life. She had gotten pregnant with Lieserl when he was waiting to be recruited to the Patent Office and she displayed irrational jealousy at his interest in a young female student when he was about to be given a professorship.

Women, it seems, were actually more trouble to him than he was to them and, of course, the only true love in Albert Einstein's life was theoretical physics.

[Author's note: Dennis Overbye is Deputy Science Editor of the *New York Times*. His first book, *Lonely Hearts of the Cosmos*, was nominated for a *Los Angeles Times* Science Book Award.]



Relational Mechanics

by André K.T. Assis
ISBN 0-9683689-2-1
$25.00 Paperback, 285 pp.
Apeiron, 1999

Review by Thomas E. Phipps, Jr.
Reprinted with permission of *Apeiron*.

The author of this remarkable book and of *Weber's Electrodynamics* (Kluwer, Dordrecht, 1994) has dedicated much of his professional life to reviving and exploiting the mathematical methods developed by Wilhelm Weber in the mid-1800s for treating electromagnetism and gravity. In a word, Weber adapted Newton's instant action-at-a-distance approach to the parallel treatment of both these topics. This was accomplished in a genuinely relativistic way—that is, by formulating point pair interaction potentials and forces as dependent only on the distance of separation of the points and on time derivatives of that separation. To define such a separation distance unambiguously requires an absolute simultaneity. For this reason the approach has had few followers since the advent of Maxwell and Einstein. But any reader of Assis' books must conclude that this eradication of pluralism in the foundations of physics constitutes a great loss to the discipline. It has inflicted a sort of tunnel vision upon the profession, but fortunately, it is never too late to reform. I can think of no better way of broadening one's physics perspective than the study of Assis' works. They bring up-to-date Weber's approach, and demonstrate that there exists an intellectually respectable alternative to the divergence-plagued methods of field theory, which have seemingly been explored to exhaustion.

In this respect the present book is particularly enlightening. In fact, I feel obliged to use the over-worked term "revolutionary." Here is indeed a new paradigm, for any who are willing to validate their claims to open-mindedness through a small investment of their time. The new paradigm is strongly Machian. As we know, Einstein began by claiming an implementation of Mach's approach in his general relativity theory. . .but soon realized the incompatibility of his own field-based causal ideas with the acausality of Mach's notion of an inertial "influence" of distant matter on present happenings.

Mach failed to quantify his conception. He made no attempt to show specifically how the "gravity" of distant matter might affect local inertial responses. This was because he did not challenge Newton's particular form of the law of

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gravity. Weber, however, did offer such a challenge. His idea, to modify the law of gravitational potential by a factor dependent on the relative motion of the interacting point masses—analogous to the velocity dependent potential by which he had successfully described Faraday induction and other effects in electromagnetism—was long ignored by physicists (owing to the advent and total dominance of field theory), but was reinvented by Schrödinger in a little-known paper [*Annalen der Physik*, 77, 325-336 (1925)] of crucial importance. In this paper he integrated a Weber-type velocity-dependent gravitational interaction over a shell of distant matter and showed that local inertial effects could thereby be explicated. That demonstration provided a hint and gave the needed impetus to Assis' much more comprehensive exploitation in the present book of what emerges as a startlingly "new" descriptive theme. . .one that gives quantitative substance to "Mach's principle."

The book presents its subject matter in two parts, a review of classical Newtonian mechanics (which is instructive even for the reader well-versed in conventional treatments) and a presentation of the new "relational" mechanics—so called because the Weber-type separation distance parameter on which it relies establishes an invariant "relation" between its two endpoints. That is, observers in arbitrary states of motion must all agree on the spatial relationship between the paired particles so described. Hence the goal of Einstein's general relativity theory is implicitly realized through the Weber parametrization of the physical descriptive problem. . . without need for tensors, metrics, manifolds, curved space, etc. To gain access to this alternative there is only one price to be paid: Einstein's epiphany of the "relativity of simultaneity" has to be de-epiphanized. To me this seems a small price.

And for that price what do we buy? Assis shows that so-called "inertial forces" (centrifugal and Coriolis) can be attributed to the Machian influence of distant matter, with the consequence that "the sum of all forces of any nature acting on any body is always zero in all frames of reference." Thus "inertial forces" are not fictitious, but are in fact gravitational in origin. This is a great simplification of the foundations of mechanics. In its sequel, frames of reference have become a mere descriptive convenience, inasmuch as the basic force laws are formulated in relational terms independent of reference frames and invariant under their changes. This invariance bears no mathematical resemblance to "covariance," a device suited to field theory, not to instant-action theory. In starting down a new road, one has to rid one's mind of old "truths," even as to the meaning of invariance. To be sure, the physics on this new road is spooky . . . the distant matter cannot be ignored, as we are used to doing, but has to be included (in principle) via integration over an assumed uniform distribution of matter at great distances beyond our galaxy.

Applications to cosmology are discussed, and gratifying agreements with observed orders of magnitude are obtained. . .but as a physicist I find these less exciting than the possibility that future satellite experiments with gyroscopes, for instance, might probe small discrepancies between Newton's theory, Assis' theory, and general relativity. (During the whole of my mature lifetime I have heard promises of such an experiment . . .but it is never more than talk. Is it possible that the physics Establishment secretly shrinks from putting the general theory to the real test of a controlled experiment? Look at the gravy trains that might run off the track. . .and unnecessarily so, when fiddling of NASA priorities can easily prevent it. But such science-fear is foolish, since fiddling of adjustable functions in the general theory is as just easy and more Nobel-worthy.)

Assis has made his point clearly and (I think) irrefutably, that Mach's principle suddenly makes quantifiable sense the moment we admit a Weberian velocity dependence of gravitational potential. But that is the lesser part of his story. The big news is that underlying Newtonian mechanics—which for three hundred years physicists have viewed as a completed intellectual creation—is a whole new discipline of Machian *pre-mechanics*. I have little doubt that what will come out of this challenge to field theoretical presuppositions will be a closer linkage between classical mechanics and quantum mechanics. The latter already displays spooky acausal features. Perhaps the creakiness at the joints between relativity theory and quantum theory, customarily seen as a shortcoming of the latter, will turn out to be curable by replacing relativistic mechanics with relational mechanics. I ask you, where can $25 buy you a better chance on a revolution? ❑ ❑ ❑

Device and Testing continued from page 43

read the real power too low by that amount, so the computer which monitors the entire measurement system calculates a false excess heat of about 0.9 watts, which has ranged from 0.4 to 1.0 watts excess heat. This means our 2 to 4 watts of excess heat results should really be 1 to 3 watts of excess heat. The numerous empty chamber tests were supported by tests with the resistive loads on the oscillator. Now we are sure that excess heat is occurring reliably, and that our test apparatus is performing well, except for the OSI error, which we do not want to tamper with; we know its error and it is consistent. We want to leave it where we know it is.

As we go to press with this issue, we are testing a new wattmeter that is custom-made by Pioneer Microsystems, Inc. of Pittsburgh, Pennsylvania. So far, the new wattmeter reads the same as the OSI reading minus the oscillator calorimeter heat loss. Piezo loading is the next level of testing. ❑ ❑ ❑

Whenever you find yourself on the side of the majority, it is time to pause and reflect.

— Mark Twain

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Yokohama, Japan

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The Ninth International Conference on Cold Fusion (ICCF9)

has been scheduled at the International Convention Center, Tsinghua University, in Beijing, China from May 19 to 24, 2002.

The study of Cold Fusion (CF) phenomena, sometimes referred to as "New Hydrogen Energy" (NHE) is progressing in numerous laboratories all around the world. More than ten years old, the field is enjoying ever more reproducible experiments and better interactions with the scientific community. A large variety of experimental approaches will be addressed at the Conference, together with theoretical attempts to interpret the observed phenomena.

The important sponsorships that have been secured for ICCF9 (Fundamental Research Division, Ministry of Science and Technology Physics Division II, Natural Science Foundation of China, Chinese Nuclear Physics Society, Department of Physics, Tsinghua University) guarantee that the Conference will maintain the most rigorous scientific standards. As stated in Lerici at ICCF8, the theme of ICCF9 will be coherence: Coherence between Nuclear Physics and Solid State Physics; Coherence between Cold Fusion and Hot Fusion; Coherence between Research and Development. We are glad to see the progress in all three aspects in the past years.

The following are some topics that will be covered at ICCF9: heat and related products; nuclear processes and transmutations; materials; innovative approaches; and theories.

The Conference will consist of oral and poster presentations. The official language of the Conference will be English.

Pre-registration, registration, and submission of abstracts should preferably be performed using the Conference website, which will be updated regularly, and will provide all necessary information and interactive procedures. Those who do not have access to the Internet are kindly requested to contact any one of the Conference Secretariats.

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Dayton Miller's Ether-Drift Experiments: A Fresh Look

James DeMeo*

Abstract

Dayton Miller's work on an Earth-entrained ether drift (1933) is nearly unknown today in large measure due to a 1955 critique by Shankland *et al.* (1955) published after Miller's death. Miller's interferometer fringe-shifts showed sidereal-cosmic variations at all times of day and seasons of the year, along the same cosmological axis. Shankland acknowledged the systematic patterns in Miller's data, but explained them away as the consequence of temperature artifacts. My review of the subject suggests this dismissal was not well-supported. Shankland misrepresented the background history (*i.e.,* only Miller obtained positive results for ether) and displayed a strong bias against ether—only passing reference was given to Miller's counter-critique to the temperature argument, which he had addressed in control experiments and successfully rebutted while alive. A very small portion of Miller's interferometer data was inappropriately dissected and criticized, ignoring the larger part of his work. Shankland *et al.* did not provide a serious challenge to Miller, and the entire subject of an Earth-entrained or dynamic ether-drift begs for renewed open discussion and experimental evaluation.

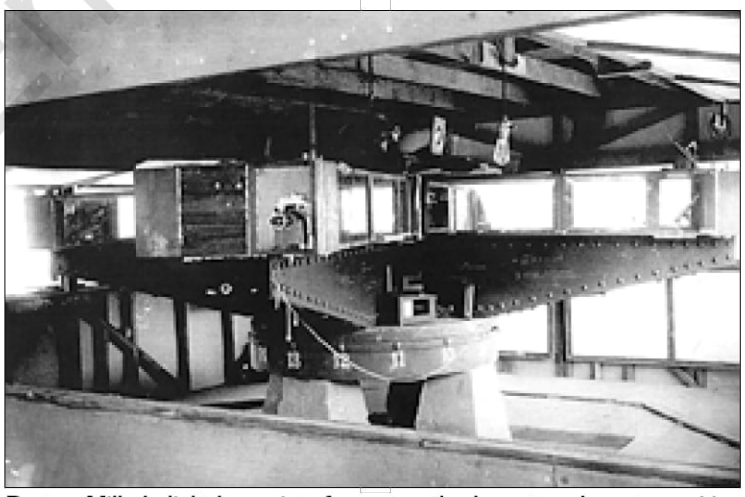
> *"The effect [of ether-drift] has persisted throughout. After considering all the possible sources of error, there always remained a positive effect."*
>
> —Dayton Miller (1928, p. 399)

> *"I believe that I have really found the relationship between gravitation and electricity, assuming that the Miller experiments are based on a fundamental error. Otherwise, the whole relativity theory collapses like a house of cards."*
>
> —Albert Einstein, in a letter to Robert Millikan, June 1921 (in Clark 1971, p. 328)

Dayton Miller's 1933 paper in *Reviews of Modern Physics* details the positive results from over twenty years of experimental research into the question of ether-drift, and remains *the most definitive publication on the subject* of light-beam interferometry. Other positive ether-detection experiments, such as the work of Sagnac (1913) and Michelson, Gale, and Pearson (1925) also are significant, but they tested purely for differences in the speed of light-transmission in different directions (c+v > c-v) using either rotating interferometers in a laboratory, or large interferometers anchored to the rotating Earth. Those experiments, as with the widely-misreported Michelson-Morley experiment of 1887, all produced positive results but were not constructed for detection of a larger cosmological ether-drift, of the Solar System moving through the background of space. Dayton Miller's work on ether-drift was so constructed, and yielded consistently positive results.

Miller's work, which ran from 1906 through the mid-1930s, most strongly supports the idea of an ether-drift, of the Earth moving through a cosmological medium, with calculations made of the actual direction and magnitude of drift. By 1933, Miller concluded that the Earth was drifting at a speed of 208 km/sec towards an apex in the Southern Celestial Hemisphere, towards Dorado, the sword-fish, right ascension 4 hrs 54 min., declination of -70° 33′, in the middle of the Great Magellanic Cloud and 7° from the southern pole of the ecliptic (Miller, 1933, p. 234). This is based upon a measured displacement of around 10 km/sec at the interferometer, and assuming the Earth was pushing through a stationary, but Earth-entrained ether in that particular direction, which lowered the velocity of the ether from around 200 to 10 km/sec.

Today, however, Miller's work is hardly known or mentioned by mainstream physicists, as is the case with nearly all the experiments which produced positive results for an ether in space. Modern physics today points instead to the much earlier and less significant 1887 work of Michelson-Morley, as having "proved the ether did not exist." While Miller had a rough time convincing some of his contemporaries about the reality of his ether-measurements, he clearly could not be ignored in this regard. As a graduate of physics from Princeton University, President of the American Physical Society and Acoustical Society of America, Chairman of the Division of Physical Sciences of the National Research Council, Chairman of the Physics Department of Case School of Applied Science (today Case Western Reserve University), and Member of the National Academy of Sciences well-known for his work in acoustics, Miller was no "outsider." While he was alive, he produced a series of papers presenting solid data on the existence of a measurable ether-drift, and he successfully defended his findings to not a small number of critics, including Einstein. His work employed light-beam interferometers of the same type used by Michelson-Morley, but of a more sensitive construction, with a significantly longer light-beam path. He periodically took the device high atop Mt. Wilson (above 6,000′ elevation), where Earth-entrained ether-theory predicted the ether would move at a faster speed than close to sea level. While he was alive, Miller's work could not be



Dayton Miller's light-beam interferometer, the largest and most sensitive such apparatus ever constructed, set up inside a special hut on Mount Wilson, c. 1925. *Photo courtesy of the Case Western Reserve Univ. Archive.*

New Energy Times Archive

fundamentally undermined by the critics. However, towards the end of his life, he was subject to isolation as his ether-measurements were simply ignored by the larger world of physics, then captivated by Einstein's relativity theory.

After his death in 1941, Miller's work was finally "put to rest," in the publication of a critical 1955 paper in *Reviews of Modern Physics* by Robert S. Shankland, S.W. McCuskey, F.C. Leone, and G. Kuerti (hereafter referred to as the "Shankland team" or "Shankland" paper), which purported to make a fair and comprehensive review of Miller's data, finding substantial flaws.

Lloyd Swenson's *Ethereal Aether* (1972) presents a cursory discussion of Miller and his "inexplicable" positive results, giving a high degree of significance to the Shankland team's critique. Swenson wrote:

> *. . .Shankland, after extensive consultation with Einstein, decided to subject Miller's observations to a thoroughgoing review . . . Einstein saw the final draft [of Shankland's pre-publication manuscript] and wrote a personal letter of appreciation for having finally explained the small periodic residuals from [Miller's] Mount Wilson experiments.* (Swenson, 1972, p. 243)

In August of 1954, Einstein replied to Shankland:

> *I thank you very much for sending me your careful study about the Miller experiments. Those experiments, conducted with so much care, merit, of course, a very careful statistical investigation. This is more so as the existence of a not trivial positive effect would affect very deeply the fundament of theoretical physics as it is presently accepted. You have shown convincingly that the observed effect is outside the range of accidental deviations and must, therefore, have a systematic cause [having] nothing to do with 'ether wind', but with differences of temperature of the air traversed by the two light bundles which produce the bands of interference.* (Shankland, 1973a, p. 2283)

From the above accounts, it certainly would appear that the case was finally closed on Miller, and that all the loose ends were finally cleaned up. With the strongest support for cosmological ether-drift swept aside as the alleged product of temperature errors, Einstein's theory of relativity continued to grow in popularity and dominance.

Here, I will compare the Shankland team's 1955 criticisms to what is actually contained in Miller's published works, notably his 1933 paper which summarized his work on the subject. It is my contention the Shankland paper, published nearly fifteen years after Miller's death, attempted to resurrect speculative criticisms which had previously been raised and rebutted when Miller was alive, and not given serious credibility except among anti-ether fundamentalists. The Shankland paper also misrepresented Miller's data in several ways, and furthermore misrepresented itself as a definitive rebuttal, which it most certainly was not. In order to properly address this major issue of science history, I will also recount the central facts of Miller's work.

The basic principles of light-beam interferometry for detection of ether-drift are described in most textbooks, albeit with typical factual errors (*i.e,* the slight positive result of the Michelson-Morley experiment is nearly always misrepresented as a "null" or "zero" result) and so will not be repeated here. However, there were novel methods introduced by Miller into the discussion of ether-drift, along with interferometer construction features and principles of operation which are not widely known—these will be detailed.

Miller's work on ether drift was clearly undertaken with more precision, care, and diligence than any other researcher who took up the question, including Michelson, and yet, his work has basically been written out of the history of science.

Miller's Work with Interferometry

Miller began his work on the question of ether-drift and light-beam interferometry with Edward Morley, from 1902 to 1906, using an apparatus three times as sensitive as the original interferometer used by Michelson-Morley in 1887. In later years, from 1921 through 1926, Miller undertook ether-drift research independently using an even more sensitive instrument with a light path of 64 meters. Miller became convinced of an ether Earth-entrainment effect, which necessitated using the apparatus at higher altitudes (to reduce the anticipated entrainment-effect of sea level environments), and he additionally undertook the experiments in structures where the walls at the level of the light-path were open to the air, covered with canvas. Only glass, or glass and corrugated paper covers, were used along the light-beam paths, with all metal shielding removed. By contrast, the original Michelson-Morley interferometer had a light path of only 11 meters, and the experiments were undertaken in the basement of one of the large stone buildings at Case School in Cleveland.

Miller's interferometer was the most massive and sensitive ever constructed, with iron cross-beams more than 4 meters across, and standing 1.5 meters in height. Four sets of mirrors were mounted on the cross-beams to repeatedly reflect light beams back and forth in a horizontal path over 64 meters long, starting from the same light-source, and finally recombined to form interference fringes whose movement relative to a pointer was read through a magnifying lens. The large apparatus was floated inside a circular tank of liquid mercury, providing a frictionless base for rotation. Fringe-shift movements (in tenths of a fringe, plus or minus in direction) were observed by one person who walked around with the apparatus while it turned, speaking out the readings at the ring of a bell which automatically sounded when electrodes made contact at 24° intervals (dividing the circle into fifteen parts). An assistant then noted the readings on paper. The readings, from consecutive turns of the apparatus were then organized into "sets," which were made at different times of day and at different seasons of year. Data sets were then averaged according to a sidereal time clock, which was correlated with external celestial coordinates.

In his 1933 paper, Miller published the most comprehensive summary of his work, and the large quantity of data which supported his conclusions. A total of over 200,000

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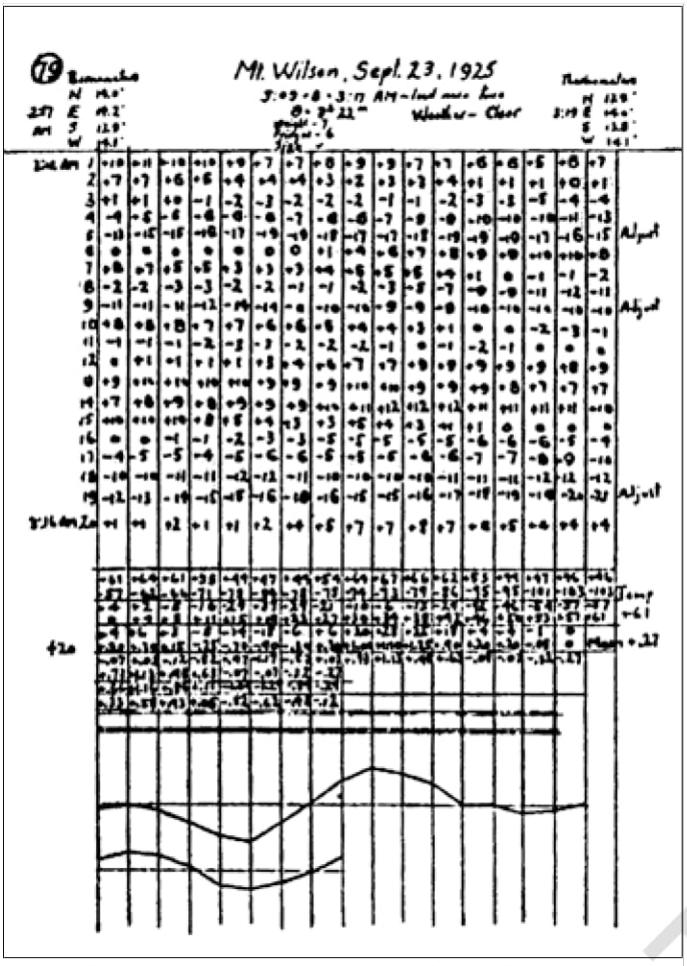
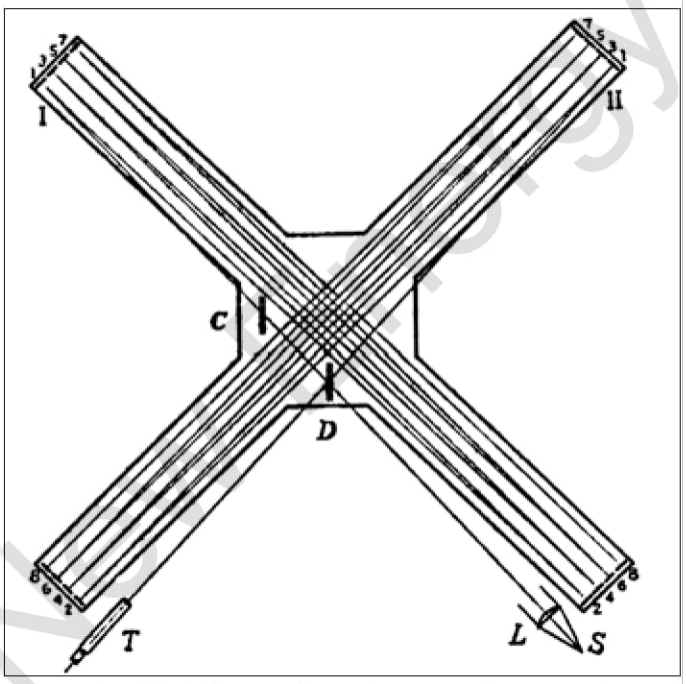


A typical data sheet recording 20 turns of the interferometer, in this case, on September 23, 1925, 3:09 to 3:17 AM at Mount Wilson.



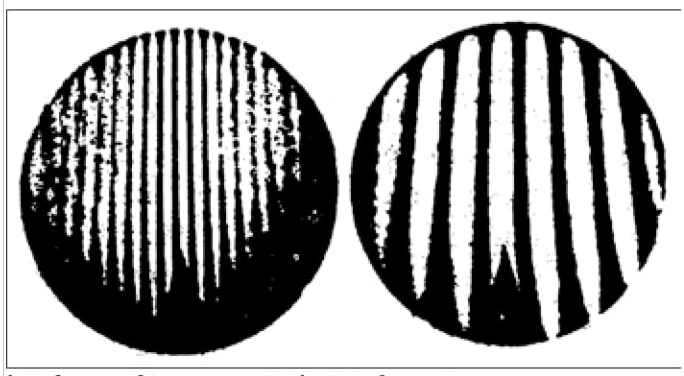
Light paths of the Miller interferometer, as seen from above. Source (S) generates light which passes through lens (L) and is then split by half-silvered mirror (D). Beams then reflect back and forth along beams (I and II) to mirrors (numbered 1-8) before finally being recombined by half-silvered mirror (D) and reflected to small telescope eyepiece (T) where interference fringes are observed.

individual readings were made, from over 12,000 individual turns of the interferometer, undertaken at different months of the year, starting in 1902 with Edward Morley at Case School in Cleveland, and ending in 1926 with his Mt. Wilson experiments. These data do not include many rigorous control experiments undertaken at Case School Physics Department from 1922 to 1924. More than half of Miller's readings were made at Mt. Wilson using the most sophisticated and controlled procedures, with the most telling set of experiments in 1925 and 1926. By contrast, we can mention here, the original Michelson-Morley experiment of 1887 involved only *six hours of data collection* over four days (July 8, 9, 11, and 12 of 1887), with a grand total of only thirty-six turns of their interferometer. Even so, as shown below, Michelson-Morley originally obtained *a slight positive result* which has been systematically ignored or misrepresented by modern physics. As stated by Michelson-Morley:

> *. . .the relative velocity of the earth and the ether is probably less than one-sixth the earth's orbital velocity, and certainly less than one-fourth. . . The experiment will therefore be repeated at intervals of three months, and thus all uncertainty will be avoided.* (Michelson-Morley, 1887)

Unfortunately, and in spite of all claims to the contrary, Michelson-Morley never undertook those additional experiments at the different seasonal configurations, to "avoid all uncertainty." However, Miller did. Over many years, he developed increasingly sensitive apparatus, using them at higher altitudes and in open structures, making clear and positive detection of the ether. His experiments yielded systematic periodic effects which pointed to a similar identifiable axis of cosmic ether-drift, though of a variable magnitude, depending upon the season, time of day, density of materials shielding or surrounding the apparatus, and altitude at which the experiment was undertaken. He argued that basement locations, or interferometers shielded with opaque metal housings, yielded the most tiny and insignificant effects, while those undertaken at higher altitudes and in less dense structures yielded more readily observable effects. The Michelson-Morley experiment, by comparison, was undertaken in the basement of a stone building closer to sea-level. Even so, it produced a slight positive result which was in agreement with Miller's results.

Miller's observations were also consistent through the long period of his measurements. He noted, when his data were plotted on sidereal time, they produced *". . .a very striking consistency of their principal characteristics. . .for azimuth and magnitude. . . as though they were related to a common cause. . . The observed effect is dependent upon sidereal time and*



Interference fringes seen in the interferometer.

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is independent of diurnal and seasonal changes of temperature and other terrestrial causes, and. . .is a cosmical phenomenon." (Miller, 1933, p. 231)

Debates with Einstein

There are several newspaper accounts indicating a certain tension between Albert Einstein and Dayton Miller, since the early 1920s at least. In June of 1921, Einstein wrote to the physicist Robert Millikan: *"I believe that I have really found the relationship between gravitation and electricity, assuming that the Miller experiments are based on a fundamental error. Otherwise, the whole relativity theory collapses like a house of cards."* (Clark, 1971, p. 328) Privately, in letters and in spoken words, there was a struggle going on for philosophical dominance, and occasionally this struggle surfaced into public view:

GOES TO DISPROVE EINSTEIN THEORY
Case Scientist Will Conduct Further Studies in Ether Drift. Einstein Discounts Experiments

Speaking before scientists at the University of Berlin, Einstein said the ether drift experiments at Cleveland showed zero results, while on Mount Wilson they showed positive results. Therefore, altitude influences results. In addition, temperature differences have provided a source of error.

"The trouble with Prof. Einstein is that he knows nothing about my results." Dr. Miller said. "He has been saying for thirty years that the interferometer experiments in Cleveland showed negative results. We never said they gave negative results, and they did not in fact give negative results. He ought to give me credit for knowing that temperature differences would affect the results. He wrote to me in November suggesting this. I am not so simple as to make no allowance for temperature."

(*Cleveland Plain Dealer* newspaper, January 27, 1926)

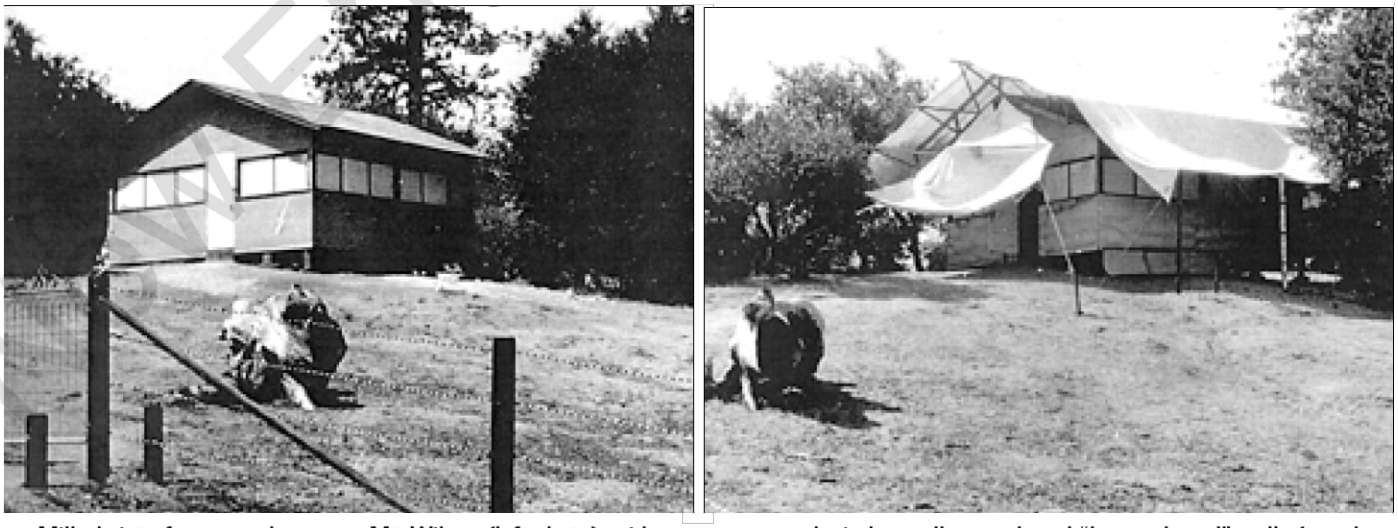
The above newspaper account is significant, as it demonstrates that Einstein was pushing the "thermal artifact" argument against Miller's results as early as 1926. There are other accounts of Einstein's discontent with Miller's results in "Conversations with Albert Einstein" written by Robert Shankland in the years after Miller's death (Shankland, 1963; 1973b).

Miller's Control Experiments

Miller was fully aware of the criticisms being made against his findings, that his interferometer was responding to one or another mechanical, magnetic, or thermal influence. Given its large size and sensitivity, it required a careful set-up procedure prior to each use. Setting screws with extremely fine threads were used to adjust the mirrors, and the final adjustment could isolate 100 wavelengths of light by just a 16° turn of the screw. Even this was insufficient for the final adjustment, which was made by adding small weights of around 100 grams to the ends of cross-beam, which was sufficient to cause a micro-flexing of the iron supports by only a few wavelengths. Only then would the interference fringes come into view. And once in view, additional care had to be taken to prevent distortions from mechanical vibrations. Consequently, from the very beginning of the ether-drift experiments, Miller undertook extensive control experiments and procedures to guard against laboratory artifacts, and to objectively determine just how sensitive his apparatus was to external influences.

In 1921, he made a special interferometer of aluminum and brass, to guard against the possible effects of magneto-constriction (the measured periodic ether-drifting was the same as with the original iron interferometer). Especially between 1922-1924, Miller's control experiments were most rigorous, aimed at addressing the criticisms he had received following the earlier work, to make the apparatus as sensitive as possible only to ether-drift. Procedures were made to judge the effects of mechanical vibration—such as using a loose or tight centering pin. Bases made of wood, metal, or concrete were floated in the mercury tank, to judge and correct for the effects of strain and deformation. The apparatus was not touched when operating, but rather gently pulled in a circle by a thin string, slowly accelerated to the desired velocity of rotation while floating in the mercury tank. Different light sources were tried, mounted on different locations on the apparatus. Light sources outside the structure were also tried, including sunlight, but finally an artificial light source located above the central axis of the instrument was used.

Possible temperature effects were evaluated by using radiant parabolic heaters to artificially heat the apparatus and the air through which the light-beam passed. These experi-

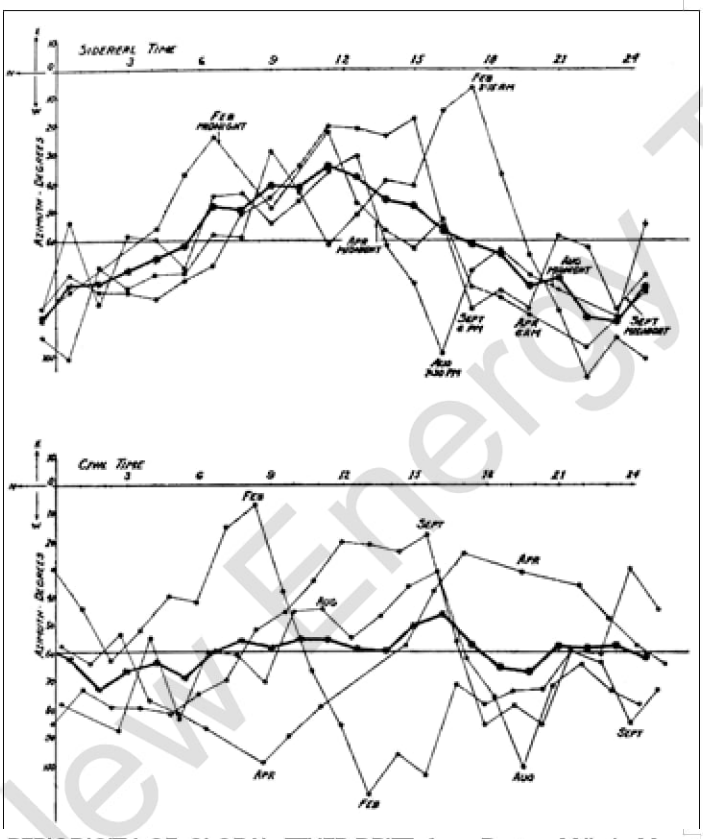


Miller's interferometer house on Mt. Wilson (left photo), with canvas-covered windows all around, and "beaver-board" walls (wood fibre composite). The right photo shows the same house fitted with a tent cover, which stabilized interior daytime air temperatures.

New Energy Times Archive

ments showed the interferometer clearly was sensitive to artificial heating, and so steps were taken to eliminate the effect. Strong radiant heat sources, it was learned, would badly skew the apparatus if focused upon only one arm or pair of arms of the iron cross-beams. Equal heating of the apparatus had no such effect, but the metal arms were nevertheless covered with a one-inch cork insulation to guard against radiant thermal effects. The light-path was given a glass housing, which stabilized the temperature inside, and later, a light corrugated paper cover was added over the glass cover, which did not affect the ether-drift, but further protected against possible temperature variations. Low-level thermal effects were also evaluated, as from human body heat, by having the recording assistant stand in different locations while the apparatus was turned and operated.

Temperature effects from the larger environment were evaluated as well. Early ether-drift experiments, including those of Michelson-Morley and Morley-Miller, were undertaken inside basement locations with relatively stabilized temperatures, but shielded from the ether-drift as well due to heavy and dense building materials. Miller's ether-drift experiments atop Mt. Wilson required a different approach, and a special house was constructed to shelter the interferometer. It had a floor, walls and roof, and canvas-covered windows all around at the level of the interferometer light-beam. During his last set of Mt. Wilson experiments in 1925-1926, a tent-like covering was erected over the roof and walls to provide additional shielding from direct sunlight, to diminish thermal variations or radiant heating effects from the walls. Miller noted at no time during his entire work on the question did he ever observe any periodic effects expressing themselves according to civil time coordinates, as would be present if a thermal effect was radiating from a specific wall. Since the measurements were made at different times of day, and at different seasons, their amplitude would vary, but the direction of the ether-drift would shift only to the same average points along a sidereal azimuth. The measurements were latitude-dependent as well, and when analyzed with attention to the Earth's rotation, axial tilt, movement around the Sun, and Sun's movement through galactic space, finally revealed a common sidereal cosmological axis of ether-drift.



PERIODICITY OF GLOBAL ETHER-DRIFT, from Dayton Miller's Mount Wilson Ether-Drift Experiments, 1925-26. Top figure, data from four separate months or epochs, measured at different times of the year, is organized by sidereal time and shows a definitive periodic curve. The heavy line is the mean of all four epochs. Bottom figure, same data organized by civil clock time, which spreads out along the graph, without apparent periodicity. This demonstrates the detected axis and periodicity of ether drift is the same for different times of year, but can only be seen when the data is viewed along a cosmological, sidereal coordinate system. (From Miller, 1928, p. 362)

From reading his publications, one gets the impression of Dayton Miller as a very careful and exceptionally patient experimentalist, someone who took every possible precaution to insure his apparatus was detecting only the phenomenon of interest. He also appeared to be quite content with the possibility that, having undertaken all the various controls to shield the apparatus from thermal effects in the measurement room, he might finally get a true "null" or "zero" effect—he did not appear to be a "believer" in ether-drift who would succumb easily to bias. He was a genuine scientist, dedicated to finding the truth of the matter. A null result was not observed, however, and his efforts to control-out mechanical and thermal artifacts never eliminated the observed periodic sidereal variations, which persisted throughout his experimental work. More will be said about Miller's control procedures below.

Michelson and Others Confirm an Ether-Drift?

Miller's work did finally receive an indirect support from Albert Michelson in 1929, with the publication of "Repetition of the Michelson-Morley Experiment" (Michelson, Pease, Pearson, 1929). The paper reported on three attempts to produce ether-drift fringe shifts, using light-beam interferometry similar to that originally employed in the Michelson-Morley (M-M) experiments.

In the first experiment, undertaken in June of 1926, the interferometer was the same dimensions as the original M-M apparatus, with a light-path of 11 meters. A fringe shift displacement of 0.017 was predicted, but the conclusions stated *"No displacement of this order was observed."* The second experiment, undertaken on unspecified "autumn" dates in 1927, used a longer light path of around 16 meters. Again, *"no displacement of the order anticipated was obtained,"* and the short report did not give details about the experimental surroundings or locations.

The third experiment was undertaken on an unspecified date (probably 1928) in *"a well-sheltered basement room of the Mount Wilson Laboratory."* They increased the light-path further still, to approximately 26 meters. This time, having moved the apparatus to a higher altitude and using a longer light-path, a small quantity of ether-drift was detected, which approximated the results observed by Miller, although the results were reported in negative terms:

> *. . . Precautions taken to eliminate effects of temperature and flexure disturbances were effective. The results gave no displacement as great as one-fifteenth of that to be expected on the supposition of an effect due to a motion of the solar system of three hundred kilometers per second. These results are differ-*

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ences between the displacements observed at maximum and minimum at sidereal times, the directions corresponding to . . .calculations of the supposed velocity of the solar system. A supplementary series of observations made in directions halfway between gave similar results.

(Michelson, Pease, and Pearson, 1929)

One-fifteenth of 300 km/sec is 20 km/sec, a result the authors dismissed as they apparently had discarded the concept of an Earth-entrained ether, which would move more slowly closer to sea level. A similar result of 24 km/sec was achieved by Kennedy-Thorndike in 1932, however they also dismissed the concept of an entrained ether and, consequently, their own measured result: *"In view of relative velocities amounting to thousands of kilometers per second known to exist among the nebulae, this can scarcely be regarded as other than a clear null result."* This incredible statement serves to illustrate how deeply engrained was the concept of a static ether.

Michelson, Pease, and Pearson went on to make speed-of-light measurements in a one mile long partially-evacuated steel tube lying flat on the ground, oriented roughly southwest to northeast. While the purpose of these experiments was not to measure any ether-drift or variation in the speed of light, such variations in fact were observed and reported in their paper (Michelson, Pease, and Pearson, 1935). A newspaper account of these experiments, published after Michelson's death in 1931 but prior to their final publication of results reported: *"Dr. Pease and Mr. Pearson say the entire series of measures, made mostly between the hours of 7 and 9 PM, show fluctuations which suggest a [variation] of about 20 kilometers per second"* (Dietz, 1933). Miller commented on these results, suggesting they would have measured a stronger ether-drift variation if they had taken their interferometers outside of the basement structures and steel pipes:

If the question of an entrained ether is involved in the investigation, it would seem that such massive and opaque shielding is not justifiable. The experiment is designed to detect a very minute effect on the velocity of light, to be impressed upon the light through the ether itself, and it would seem to be essential that there should be the least possible obstruction between the free ether and the light path in the interferometer.

(Miller, 1933, p. 240)

Miller had, by this time, acquired a lot of experience working on Mt. Wilson, using his large interferometer in the specially-constructed interferometer house. With a light path of 64 meters, Miller's apparatus was still 2.5 times more sensitive than the best apparatus of Michelson-Pease-Pearson. Given that Michelson-Pease-Pearson did make some small detection of an ether-drift from their efforts at Mt. Wilson, in spite of the fact that it was located in a basement location, their report of detectable sidereal fringe displacements supports Miller's findings. It is also notable that this was the second time Michelson's work had detected an ether, though in the first instance of Michelson and Gale (1925) the apparatus could only measure light-speed variations along the rotational axis of the Earth. These papers by Michelson, and by Kennedy-Thorndike, have conveniently been forgotten by modern physics, or misinterpreted as being totally negative in result, even though all were undertaken with far more precision, with a more tangible positive result, than the celebrated Michelson-Morley experiment of 1887. Michelson went to his grave convinced that light speed was inconstant in different directions, and also convinced of the existence of the ether. The modern versions of science history have rarely discussed these facts.

Shankland Team's 1955 Critique of Miller

As previously pointed out by Swenson, Shankland's 1955 critique of Miller's work was undertaken with "extensive consultations" with Einstein, who like Newton and others before him had assumed only a static or stagnant ether, through which the Earth passed without material affect and, hence, without entrainment close to the Earth's surface. Shankland in fact was Miller's student for many years, and only emerged to become a professional advocate of Einstein's relativity after the death of Miller in 1941. Shankland became Chairman of the Physics Department at Case following Miller's retirement and death, building his professional career upon publications misrepresenting the Michelson-Morley experiments as the most solid evidence on the question, and publishing widely-read interviews with Einstein (Shankland, 1963, 1964, 1973a, 1973b). Shankland later took up administrative positions within government agencies developing nuclear energy—he rarely discussed Miller's positive ether-drift measurements in any of these papers except in the 1955 paper under discussion here. In this sense, it is legitimate to view Shankland, and other members of his team (all Einstein advocates from Case), as very biased reviewers of Miller's work.

The very first sentence in the Shankland team's 1955 paper began with the falsehood, now widely parroted in nearly every physics textbook, that the Michelson-Morley experiments had a "null" result. The third sentence in the Shankland paper was similarly false, claiming that *"All trials of this experiment except those carried out at Mount Wilson by Dayton C. Miller yielded a null result within the accuracy of the observations."* This kind of chronic misrepresentation of the slight positive results of many interferometer experimenters, including Michelson-Morley, Morley-Miller, Sagnac, Michelson-Gale, and Michelson-Pease-Pearson, suggests an extreme bias and deliberate misrepresentation. The fact that this is a very popular bias does not excuse it. By redefining all the positive results observed by what may in fact have been the majority of ether-drift researchers, as mere expressions of "observational inaccuracy," Shankland narrowed his task considerably.

These and other sentences in the Shankland paper revealed its bias from the get-go, and gave it the spirit of an autopsy, where Miller was dissected without careful concern, and certainly where no advocate of ether theory appeared to be involved in the process. It is possible that by the 1950s there was nobody left who could fill Miller's shoes to make an adequate defense. Ether-theory was then being compared to "the search for perpetual-motion machines" (Swenson, 1972, p. 239), and such ridicule surely must have had a silencing effect upon the entire fields of physics and astronomy. Swenson also suggests that, during his later years, Miller was largely ignored and isolated. This appears to be correct, as according to an interview with Shankland made in 1981, shortly before Miller died he gave all of his interferometer data sheets—hundreds of pages of measurements—to his one-time student Shankland, with the somewhat bitter statement that he should "either analyze the data, or burn it" (Kimball, 1981, p. 2). In that same inter-

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view, Shankland also blamed Miller for having blocked the awarding of a Nobel Prize to Einstein for his relativity theory—clearly, Miller's work was a major obstacle to the Einstein theory of relativity, and for that reason may have given Einstein and his followers sleepless nights.

The title of the Shankland paper, and its overall representation suggests the authors had made a serious review of *"the* interferometer observations" of Miller, to include some kind of comprehensive and inclusive evaluation—but this was not the case. There were two basic approaches to the Shankland team's analysis: 1) a search for random errors or statistical fluctuations in Miller's data, and 2) a review of selected data sets which they claimed demonstrated significant thermal artifacts in the data. We can review these claims.

Shankland Team's Evaluation for Random-Statistical Variations

The Shankland paper did present a statistical analysis of a portion of Miller's published 1925-1926 Mt. Wilson data, concluding that his observations *". . .cannot be attributed entirely to random effects, but that systematic effects are present to an appreciable degree"* and that *"the periodic effects observed by Miller cannot be accounted for entirely by random statistical fluctuations in the basic data"* (p. 170). Also, the Shankland team admitted they *". . .did not embark on a statistically sound recomputation of the cosmic solution, but rather [looked for]. . .local disturbances such as may be caused by mechanical effects or by nonuniform temperature distributions in the observational hut"* (p. 172). In short, they admitted the harmonic patterns in Miller's data could not be due to any systematic measurement error, nor result from any mechanical flaws in the interferometer apparatus itself—while simultaneously admitting a disinterest in computation of any potentially validating ether-drift axis ("cosmic solution") from his data. These were important admissions, as the suggestion is, unless they could find some other fatal flaw in his data, Miller had really got it right, and measured a real Earth-entrained ether drift.

Of interest from the perspective of the politics of science is the fact that *this statistical analysis was not undertaken by any of the four members of the Shankland team listed as authors of the paper!* The analysis was in fact undertaken by Case physics student Robert L. Stearns, for his Master's Thesis (Stearns, 1952)—Stearns was given only a footnote credit in the Shankland paper.

Stearns, who performed the analysis, informs us about the large amount of data gathered by Miller. He mentions (Stearns, 1952, p. 15-17) the existence of "316 sets of data. . .by Miller in 1925-26" for the centrally-important Mt. Wilson experiments. Each data set was composed of twenty turns of the interferometer, with sixteen data points per turn (a total of 320 data points per data set). Miller noted his work at Mt. Wilson was undertaken at four different seasonal "epochs," each of which encompassed a period of around ten days, centered on the following dates: April 1, August 1, and September 15, 1925 and February 8, 1926 (Miller, 1926, 1933). It must be kept in mind that these Mt. Wilson data from 1925 and 1926 provided the most conclusive and foundational observations for Miller's ether-drift calculations and conclusions, as presented most clearly in his 1933 paper. As detailed below, the Shankland team mentions these Mt. Wilson data, but in a manner which confuses them with his earlier and less significant efforts, including various control experiments conducted at Case School. The significance of this confusion of dates will be highlighted momentarily.

Shankland Team's Assertion of Temperature Artifacts

Regarding possible temperature artifacts in Miller's data, this objection was raised early on in the history of ether-drift interferometry, and specifically rebutted by Miller when he was still alive. A letter exchange between Miller and Georg Joos from a 1934 issue of *Physical Review* records part of this debate, and appears to be one of the few *published* criticisms on the temperature issue Miller ever received while still alive. Miller had this to say about the problem:

> *When Morley and Miller designed their interferometer in 1904 they were fully cognizant of this. . .and it has never since been neglected. Elaborate tests have been made under natural conditions and especially with artificial heating, for the development of methods which would be free from this [thermal] effect.* (Joos and Miller, 1934)

The Shankland critique never made any systematic evaluation of possible thermal artifacts using a larger set of Miller's data, as was done with the statistical evaluation. Instead, they appear to have "gone fishing" in Miller's data for something by which they could simply dismiss him. For example, Miller's own 1923 temperature-control experiments were brought into discussion, where radiant parabolic heaters were used to artificially create a general doubling of the size of interference fringes. Miller describes these experiments:

> *Several electric heaters were used, of the type having a heated coil near the focus of a concave reflector. Inequalities in the temperature of the room caused a slow but steady drifting of the fringe system to one side, but caused no periodic displacements. Even when two of the heaters, placed at a distance of three feet from the interferometer as it rotated, were adjusted to throw the heat directly on the uncovered steel frame, there was no periodic effect that was measurable. When the heaters were directed to the air in the light-path which had a covering of glass, a periodic effect could be obtained only when the glass was partly covered with opaque material in a very nonsymmetrical manner, as when one arm of the interferometer was completely protected by a covering of corrugated paperboard while the other arms were unprotected. These experiments proved that under the conditions of actual observation, the periodic displacements could not possibly be produced by temperature effects.* (Miller, 1933, p. 220)

Perhaps without intending to do so, after examining Miller's laboratory notes for the Cleveland temperature control experiments, the Shankland team confirmed Miller on this point:

> *In the experiments where the air in the optical paths was directly exposed to heat, large second harmonics (0.35 fringe for one heater, and about twice this value for two heaters) were always observed in the fringe displacements, and with the expected phase. Shifting the heaters to a different azimuth produced a corresponding change in the phase of the second harmonics.* ***When the optical paths and mirror supports were thermally insulated, the second harmonics were greatly reduced to about 0.07 fringe.*** (Shankland *et al.*, 1955, p. 174; emphasis added, J.D.)

This statement confirmed the wisdom of Miller's approach. The added insulation reduced the thermal effects

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from a nearby radiant heater to only 20% of the uninsulated readings. I have an ordinary commercially-available electric radiant parabolic heater at my home, and it gets so hot you cannot stand closer than 12" without burning yourself, or possibly catching your clothing on fire. If Miller had used a parabolic heater even half as strong as this, it would certainly have been a source of heat much stronger than anything present in his Mt. Wilson experiments, particularly at night, during foggy or overcast conditions, and when the entire interferometer house was covered over with a tent, with the apparatus and light-beam path covered with cork, glass, and cardboard insulation. Consider a radiant heater at several hundred degrees C, creating a steep thermal gradient but only a 0.07 fringe shift in the insulated interferometer. How much *less* of an effect would be produced by a human body, or even from the inside of a solar-heated wall? Assuming an environmental thermal effect only one-tenth that seen with the parabolic heater (a wood composite wall radiating inside the structure at perhaps 50˚C?), fringe shifts of *only 0.007* would have been produced, *well below observational detection*. Miller's data sheets, for example, recorded observations *"in units of a tenth of a fringe width,"* with accuracy approaching a hundredth of a fringe only after mathematical averages of many readings were extracted.

The Shankland paper nevertheless used these control experiments as a weapon against Miller, claiming without evidence that heater-type effects *might* have occurred in his Mt. Wilson experiments, even where no such heater or remotely similar heat source was present. But why would the Shankland team shy from undertaking a more systematic evaluation for temperature artifacts? They could have, for example, evaluated only Miller's daytime interferometer experiments, and looked for a thermal effect from the southerly wall of the structure during the various epochs—if they could have shown an effect present in daytime data which was not present at night, it would have devastated Miller's claim, and proved their case. However, this obvious analytic procedure was not done, or if it was done, not reported.

The Shankland paper also resurrected the temperature criticisms by Joos (1934), but without reference to Miller's rebuttal in the same published exchange. If the periodic effects observed by Miller were the product of temperature variations, as was claimed by Shankland and Joos, then why would that variation systematically point to the same set of *azimuth coordinates* along the celestial sidereal clock, but *not* to any single terrestrial coordinate linked to civil time? Miller repeatedly asked this question of his critics, who had no answer for it. The Shankland team likewise evaded the question.

It is clear Miller had been deeply engaged on the problem of temperature effects, and worked hard to know exactly how they might be produced, and how to eliminate them. The Shankland paper, however, seized upon Miller's open acknowledgment of fringe-shifts from air heating by powerful radiant heaters during control experiments, and a few other sentences written in his lab book, and tried to claim thermal anomalies were probably the source of whatever periodic effects were subsequently measured by Miller at Mt. Wilson, when no radiant heaters were used, and when the empirically-developed control procedures were put in place. Without some kind of independent experimental evidence to support such a claim of a thermal influence, their dismissal was illogical.

The Shankland paper also went through a series of arguments about the interferometer house, how the wall materials, roof angles, interferometer glass housing, etc., might result in a definable effect upon the air temperature in the light beam path, concluding only they could not rule out such an influence—that it *". . .is not in quantitative contradiction with the physical conditions of the experiment"* (p. 175). Given their ignoring the sidereal nature of the periodicities, this statement could hardly be taken seriously, and certainly did not constitute a rebuttal of Miller's data.

The Shankland paper finally attempted to correlate several selected daytime interferometer runs with temperature measurements made at the same time. They acknowledged difficulty in correlating low fringe-shift values with low temperature differentials, but found one set of high fringe-shift values correlated with slightly higher temperatures, even while noting another set where high values correlated with lower temperatures. Finally, they complain that *". . .no temperature data are available to reveal thermal conditions at the roof, which may be responsible for the large fringe displacements at the times of highest altitudes of the Sun"* (p. 176). If this sounds confusing, a reading of the full original text provided little clarification.

Failing to show anything damning from daytime data sets, when temperature gradients inside the interferometer house might be expected to be at a maximum, they turned their focus to nighttime data sets. Once again, only a few of Miller's data sheets were selected out to prove their case. Data from two nights (August 30, 1927 and September 23, 1925) with stable air temperatures were reviewed—these nights showed very clear and systematic fringe variations (Figure 4, p. 176), but because the azimuth of the fringes changed minimally over the approximate 5 hours of observation, the critics complained *"it would be extremely unlikely if the fringe shifts were due to any cosmic effect"* (p. 177). Apparently, the Shankland team was so locked into the older "static ether" assumptions of the original Michelson-Morley experiment, they were unclear about what they should have seen in Miller's data. In 1927, at a conference on the Michelson-Morley Experiment held at Mt. Wilson Observatory, where Michelson, Lorentz, Miller and others made presentations and engaged in open debate, Miller addressed this question: *"Observations were made for verifying these [static ether] predictions . . .but it did not point successively to all points of the compass, that is, it did not point in directions 90˚ apart at intervals of six hours. Instead of this, the direction merely oscillated back and forth through an angle of about 60˚. . ."* (Miller, 1928, p. 356-357). The reason for this is, Miller's detected axis of ether-drift is oriented reasonably close (within 60˚) to both the Earth's axis of rotation and the axis of the plane of the ecliptic.

Another important fact which nearly escapes detection in the Shankland paper is that the August 30 data were made in Cleveland, while the September 23 data were from Mt. Wilson, and *neither were a part of the published Mt. Wilson data Miller used for calculations of the ether-drift*—both dates are well outside of the 10-day epochal periods identified by Miller. Furthermore, not all of the interferometer data sheets for a given date—which presumably would have had similar weather and temperature conditions—were included by the Shankland team for critical review. They selected only those data sets which appeared to support their argument of a claimed thermal anomaly. For example, they selected *"ten sets of observations, Nos. 31 to 40 inclusive, made in the hut on the Case campus between midnight and 5:00 AM*

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on August 30, 1927" and *". . . runs 75 to 83 inclusive taken from 12:18 AM to 6:00 AM on September 23"* (p. 176-177). Other than making the *claim* these selected data gave them the *impression* of being the result of temperature errors, they had no other stated criterion for bringing them into discussion. This biased data-selection, or rather *data-exclusion,* procedure forces one to ask: *What about data sets No. 1 to 30, and runs 1 to 74?* Similar unexplained data selections or data exclusions occur throughout the Shankland paper, leaving one to wonder if the *unselected and excluded* data, which constituted the overwhelming majority of it, simply could not provide support for their criticisms. One can imagine the howl of protest which would have occurred if Miller had taken this approach, arbitrarily excluding data from his calculations which superficially suggested something other than a real ether-drift.

A third data set from July 30, 1925 was highlighted by the Shankland team as it contained one extremely large peak where Miller noted, "Sun shines on interferometer." This data does appear to have been a part of Miller's published Mt. Wilson analysis. However, the Shankland team extracted only *"observations Nos. 21 to 28 inclusive, made between 1:43 AM and 6:04 AM on July 30, 1925."* Obviously, at around 6:00 AM the Sun rose and caught Miller and his assistant off-guard. What about observations Nos. 1 to 27, or other early-morning data, where the Sun *didn't* shine on the interferometer? These other data were not brought into discussion, except they did note that the runs prior to the sunshine incident demonstrated *". . .an extremely erratic behavior. . .we have no ready explanation for this apparent departure. . ."* Here, the Shankland team basically confesses their grab-bag of "ready" explanations was empty, and the idea that those data were expressing a real ether-drift was simply too "impossible" for them to consider. The fact that Miller included the note about the sunlight on this data sheet speaks to his honesty.

The Shankland team also identified data sets Nos. 56-58 from July 8, 1924, which was part of Miller's control experiments made in a basement location at Case physics laboratory—the temperatures were very stable, and the fringe oscillations were quite small, and they argued these data were a proof for thermal effects on the apparatus. However, it was this very problem of basement and dense surrounding materials which led Miller on the path to use the apparatus in locations not subject to significant ether-shielding or Earth-entrainment. After 1921, Miller only used the Case School laboratory to undertake control experiments, and that is why those particular data were never published.

The Shankland paper concluded its temperature criticisms by discussing a few additional data sets: Nos. 113-118 from April 2, Nos. 88-93 from August 8, 1925, and Nos. 84-91 from February 11, 1926 (p. 177). Here, the amplitudes and phases were claimed to have been "nearly alike," but insufficient detail was given to allow a review of the critic's claims, and it did appear they were once again incorrectly misinterpreting Miller's data along the lines of static ether assumptions.

As in almost all the cases given above, *none of these data were analyzed systematically,* nor were they presented in such a manner that the author's criticisms could be factually reviewed. I got the impression they simply scanned through a pile of Miller's data sheets, and with a wave of the hand, picking and pointing to only selected parts, dismissed it all as the product of thermal artifacts. Miller's detailed control experiments were basically ignored, as was the fact that, for all these experiments, the interferometer was enclosed in a small house covered over with a tent, while apparatus was shielded with cork insulation, and the light-beam path covered with glass and cardboard panels—with a full rotation occurring in less than a minute, one is left to wonder how any observable thermal variations could develop within Miller's data, especially variations with a sidereal-cosmic component.

For the casual reader, who had not undertaken a careful review of Miller's original experiments, the Shankland paper might appear to make a reasoned argument. However, the Shankland paper basically obfuscated and concealed from the reader most of the central facts about what Miller actually did, and in any case was so unsystematic and biased in its approach, excluding from discussion perhaps 90% or more of Miller's extensive Mt. Wilson data, as to render its conclusions meaningless.

As a final note, I must regrettably inform the reader that my own search of available archive materials for both Miller and Shankland at Case University failed to discover even a single one of the hundreds of missing data sheets or laboratory records from Miller's years of hard work. Perhaps Shankland finally did burn them?

Conclusions

My review of this important chapter in the history of science left me both astonished and frustrated. Miller's work on ether drift was clearly undertaken with more precision, care, and diligence than any other researcher who took up the question, including Michelson, and yet, his work has basically been written out of the history of science. When alive, Miller responded concisely to his critics, and demonstrated the ether-drift phenomenon with increasing precision over the years. He constantly pointed out to his critics the specific reasons why he was getting larger positive results, while others got only small results, or no results. Michelson and a few others of the period took Miller's work seriously, but Einstein and his followers appeared to view Miller only as a threat, something to be "explained away" as expeditiously as possible. Einstein in fact was catapulted into the public eye following the end of World War II. Nuclear physics was then viewed as heroic, and Einstein fast became a cultural icon whose work could not be criticized. Into this situation came the Shankland team, with the apparent mission to nail the lid down on Miller's coffin. In this effort, they nearly succeeded.

The Shankland conclusions against Miller were clearly negative, but the one systematic statistical analysis of his Mt. Wilson data merely confirmed what Miller said all along, that there was a clear and systematic periodic effect in the interferometer data. The Shankland paper also confirmed Miller's contention that this periodic effect was *not the product of random errors or mechanical effects.* The Shankland team subsequently searched for temperature artifacts in Miller's data, but failed to undertake any systematic analysis of his centrally-important Mt. Wilson data in this regard. Instead, they made a biased selection of a few published and unpublished data sets obtained from different periods in Miller's research, from different experimental locations, including from his control experiments at Case.

Miller's most conclusive 1925-26 Mt. Wilson experiments encompassed a total of 6,402 turns of the interferometer,

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Photo courtesy of the Case Western Reserve University Archive.

Dayton Miller (left) and Albert Michelson (right) at a conference on the Michelson-Morley Experiment held at Mount Wilson Observatory, February 1927.

recorded on over 300 individual data sheets. That was the data the Shankland team should have been focused upon and evaluated systematically. Instead, only a few of Miller's data sheets from these most centrally-important experiments were selected—certainly less than 10% of the data available to them was brought into discussion—and then only after being firstly dissected to extract only those data which could most easily be misconstrued as evidence for presumed temperature anomalies. For certain, some of the data held up for public critique came from Miller's control experiments at Case, or possibly from trial runs when technical "bugs" were being worked out in the apparatus and building. Miller is no longer alive to inform us about his data, but the Shankland team willy-nilly lumped together both published and unpublished data, without comment.

Even though they were content to pick and choose data as they wished, they could not come up with a coherent and solid critique by which Miller's work could be conclusively dismissed—some of the data they selected merely confirmed Miller, though the Shankland group seemed ignorant of the basic ether-drift astronomy by which such an interpretation could be made. When alive, Miller openly stated he had addressed and corrected for thermal effects upon the apparatus, and yet the periodic elements of his measurements persisted—the Shankland paper ignored Miller on this important point.

The Shankland group undertook no new experiments of their own, neither on the question of ether-drift, nor on the subject of thermal perturbations of light-beam interferometry—they made essentially an "armchair analysis" of Miller's data. Only *some* of Miller's original data was carefully selected to make a rather unbelievable claim that small natural ambient temperature gradients in Miller's Mt. Wilson observation hut *might* produce fringe shifts in the insulated interferometer similar to what Miller himself previously observed in his control experiments using strong radiant heaters. The Shankland paper argued there *must have been* "thermal effects" in Miller's Mt. Wilson measurements, but provides no direct evidence of this.

At no time did the Shankland group present evidence that temperature was a factor in creating the periodic sidereal fringe shifts observed by Miller in his published data, even though this was their stated conclusion. In fact, they presented evidence from Miller's own lab notebooks which implied thermal gradients in the Mt. Wilson interferometer house would have been *below the observational limits* of the insulated apparatus.

The larger issue of periodic or harmonic effects in the data, expressed in nearly identical cosmic sidereal coordinates at different seasons and at all hours of the day, was never addressed or evaluated by the Shankland group. Neither was any attempt made to show *exactly how* an external temperature phenomenon could affect the interferometer readings to yield such a systematic sidereal effect. This issue was almost totally avoided by the Shankland team.

A reading of Miller's 1933 paper shows the picayune and biased nature of the Shankland team procedure, as the systematic sidereal periodicities observed by Miller expressed themselves nearly uniformly across the board, though at differing magnitudes. From 1906 to 1926, Miller undertook over 200,000 separate readings, over 12,000 turns of the interferometer demonstrating harmonic periodicities constantly pointing to the same general axis of ether-drift in the cosmos—a factor which was completely independent of the time of day, or season of year in which the experiments were undertaken. At best, the critics provided only an *ad-hoc argument,* a claim or suggestion without substance, that some small part of Miller's data *might* contain an undefined temperature effect.

From all the above, it appears the Shankland group, with some degree of consultation with Einstein, decided that "Miller must be wrong" and then set about to see what they could find in his archive that would support that conclusion—which is not a scientific method.

As I have discussed previously, Miller found the ether-drift effect to be stronger at higher altitudes and also to be small when the experiment was undertaken in heavy stone buildings or when the interferometer light-path was encased in metal shielding. In my studies over the last thirty years, I've found many examples from the fields of biology, meteorology, and physics that independently support the assertion of a subtle energetic force with similar altitude-dependent and metal-reflective properties (DeMeo, 1979, 1989, 1991, 1996). Likewise, there are many new findings in astrophysics, where anisotropy of cosmological factors have been discovered which are congruent with Miller's identified axis of ether-drifting (Miller, 1933, p. 241; Allais, 1997). Whereas most of these phenomenon are today interpreted as the consequence of the "Big Bang" theory, or of Einstein's relativity theory, they may also find an explanation rooted in dynamic drifting within an energetically-rich cosmological medium.

To close, I ask the reader to imagine that Michelson-Morley's 1887 experiment, which ran over only 6 hours on four days, had resulted in a claim that "the ether has been detected," and that Dayton Miller had undertook his years of work with 200,000 observations showing "the ether cannot be detected." It does not take much consideration to conclude that—in such a fictional case—Miller would today be cited in every physics textbook as having "proved the ether did not exist," and nobody would refer to Michelson-Morley. The fact that the present-day situation is totally opposite of my example is a testament to the intensely political nature of modern science, and how major theories often develop into belief systems, which demand the automatic suppression of any new finding which might undermine the faith

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and "popular wisdom" of politically-dominant groups of academics. And that "wisdom" today is: Space is empty, and the universe is dead. I submit that both are unproven, and even disproven assertions, challenged in large measure by Dayton Miller's exceptional work on the ether drift.

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Accusations of Anti-Semitism as a Barrier to Progress in Physics

John E. Chappell, Jr.*

Several years ago, a member of the Natural Philosophy Alliance (NPA), whom I will designate as "M," revealed to an old and close friend that he had just published a book claiming that Einstein's special relativity theory (SR) is invalid. "Oh," the friend replied at once, "I didn't know you were anti-Semitic."

The friend happened to be Jewish. With earnest solicitation, "M" quickly assured him that no anti-Semitism was involved; and their friendship was not damaged.

"M" keeps telling this story, in order to illustrate a very troubling and common phenomenon in the experience of critics of Einstein's special and general relativity theories: that criticism of these theories often evokes charges of anti-Semitic prejudice.

That those making such charges are not always Jewish, and that they may imply rather than openly state their suspicions of anti-Semitism, was illustrated by my experience at the International Congress on History of Science in Moscow, USSR, in 1971. In the discussion period following my paper criticizing SR, I received about five or six questions from the overwhelmingly Russian audience, and all were politely expressed. The only other questioner was an American, whom I will call "H."

In bitter and hostile tones, "H" asked me just why I had chosen this time and place to express my criticisms of Einstein's theory. I replied that I had simply come to where the congress was being held this year, the first year I could afford to travel abroad to attend one; and that I was anxious to come to Russia because for many years I had studied Russian culture, including the language, without so far visiting it. "H" merely repeated his darkly suspicious question, and sat down. I gathered that he suspected I had chosen to speak against SR in Moscow because there has been so much explicit anti-Semitism in Russia, since well before the Bolshevik revolution. (Also, while Stalin was alive, Einstein was often attacked in Soviet Russia, more on ideological than on scientific or philosophical grounds, as a "corrupt bourgeois idealist.")

Later, I approached "H" and started to explain my unprejudiced attitude towards Einstein. After a minute or two, in the middle of one of my sentences, "H" frowned, turned around, and rudely walked away.

Judging by his name and his appearance, "H" was not Jewish, at least not ethnically. He did happen to hold a prestigious official position in the History of Science Society, the chief academic organization in the U.S. for this discipline.

So it was not at all surprising to me that this encounter seemed to reinforce the heavy bias in history of science against me that had prevailed ever since I read a paper in favor of Sagnac and in opposition to SR, at a regional meeting in the U.S. in 1965 (published later that year in the more open-minded atmosphere of France, in *Archives Internationales d' Histoire des Sciences,* vol. 18). Never before my trip to Moscow, and never after it, did I ever obtain even so much as an *interview* for a job in history of science, anywhere, despite holding full Ph.D. credentials involving study under two of the most prestigious scholars in the field—Thomas Kuhn in physical sciences and John C. Greene in biological sciences.

Thus, clearly an accusation, insinuation, or even suspicion of anti-Semitism can carry with it severe penalties. Just how severe, the world in general has yet to be told. Recently the NPA obtained convincing evidence that as early as the late 1920s, physics faculty members in a major U.S. university were being fired for not accepting Einstein's special and general relativities; and at least strong indirect evidence indicating that the grounds for dismissing them involved charges of anti-Semitism.

As to my situation, it is difficult for me to imagine anyone more firmly opposed than I am, and have been ever since early boyhood, to anti-Semitic attitudes, to blanket criticisms of the Jewish people of any kind. Not only did my first distinct awareness of the larger world beyond my home and neighborhood develop in the early 1940s, when my nation became engaged in a bitter struggle against Hitler's Germany, and my first concept of what extreme evil is all about involved Naziism; but even before this war broke out, my maternal grandfather, a Slavic Pole from Chicago, who lived right next door to me when I was four to seven years old, began teaching me very explicitly and memorably that I should always be tolerant of every minority, and especially of Jews, who were rather abundant in our neighborhood on the west side of Los Angeles.

One of my three friends whose names I still remember from my first two and a half years in school (until my parents moved to another neighborhood and school district) was a Jewish boy named Herman Schwartz, one of whose grandfathers was a good friend of my own Polish grandfather.

When at the age of twenty I first turned my attention to SR—not because I was majoring in physics but simply because I believed that a well-educated person ought to know something about such a famous theory—it never occurred to me for a single moment, as I read about "time dilation" and "curved space" and quickly developed doubts that such concepts could apply to the real world, that my motives were anti-Semitic, or in any way hostile to Einstein the person. Only several years later did I begin to encounter the occasional implication or accusation of anti-Semitism; and these at first puzzled more than troubled me, so utterly remote from reality as they were.

Not long after this first investigation of SR, I began to get involved in the civil rights movement. First I worked and wrote on behalf of African-Americans, and some years later on behalf of Hispanic farm workers. (See my article on agricultural and water problems in the Lower Colorado River Basin, in the British *Geographical Magazine,* Vol. 56, 1974.) In various contexts I have continued to strive ever since for tolerance toward all minorities, in line with my grandfather's counsel.

When in the early 1990s I began to get involved in meetings with critics of modern physics, which soon evolved into the founding of the NPA and to my role as its Director, my experience made me quite alert to the need to avoid any hint

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of anti-Semitism in our ranks. Occasionally I would ask some member who went to a dissident physics meeting in Europe that I did not attend, to watch for evidence of anti-Semitism, and report back to me about it. After all, it is well-known that there was considerable anti-Semitic prejudice against Einstein in Europe, even prior to the 1930s; he himself recognized and commented on it (*e.g.,* see Ludwik Kostro, *Einstein and the Ether* [Montreal: Apeiron, 2000], p. 83). But in no instance did such inquiries, or any other probing, reveal any evidence of contemporary anti-Semitism among European dissidents—at least not on the surface of things.

Of course, I also kept a watchful eye out for such prejudice among NPA members here in the U.S. The only time I found any was not with a regular member, but with a "subscriber," or anonymous member. This person, whom I shall call "X," actually read a paper, a rather competent one, at an NPA meeting. Only months later did he suddenly break out, when talking to me on the phone, with accusations that the Jewish lobby had been advertising the Holocaust in Europe during World War II far more than it deserved, to the detriment of learning about other equally evil Holocausts such as those in Uganda and Bangladesh.

I replied that of course we need to oppose and remember *all* such horrible events, but that this does not mean that Hitler's Holocaust should be forgotten.

After he made more remarks along such lines, I told "X" that we would need strong assurances that he was not anti-Semitic, before we could allow him to speak at another NPA meeting. He responded with charges of "censorship," and sent me, among other material critical of Jews, literature from a group that claims there never was any European Holocaust during World War II at all. Eventually I gave up communicating with "X," and he did not renew his subscribership.

Then for the November 1999 issue of the *NPA Newsletter,* in a section titled "Political extremism intruding into our scholarship," I wrote that the NPA needs to suppress anti-Semitism and other forms of racial and ethnic hatred, because "in a civilized democracy, freedom of expression should be allowed only insofar as it does not damage others" (p. 6).

So we in the NPA have experienced, weathered, and repulsed our one unexpected intrusion of anti-Semitism. I plan to be at least as firm if we experience another. We must, of course, remain free to criticize the scholarship of any Jew, as well as certain policies of the Israeli state; but we must never at the same time hold or utter group-wide condemnations that amount to racial or ethnic prejudice.

Throughout this dissident struggle, I have made a point of trying to pre-empt charges of anti-Semitism, as well as simply to tell the full story about Einstein, by crediting him with doing excellent science outside of his two problematic theories; and of holding admirable views, generally humane and pacifist, in the social and political realms.

We can do still more in this area, by crediting Einstein with more flexible and tolerant approaches to science, than those typically practiced by most of his followers. For example, his considerable attention to and support of aether theories, mostly between the 1910s and 1930s, illustrates how remarkably diverse and open-minded his thought was. (See Kostro, *op. cit.*)

I suspect that in the grand scheme of things, it may actually have been a good thing for the human race in general that Einstein enjoyed such popularity and respect as he did during the period of Nazi aggression—even as it was a good thing that the victories of athletes such as Jesse Owens and Joe Louis were helpful in counteracting claims of Aryan racial superiority. After all, the world can get along for some time without a more rational theory of the physical universe, but it cannot escape destructive war without maintaining a high degree of tolerance among human groups.

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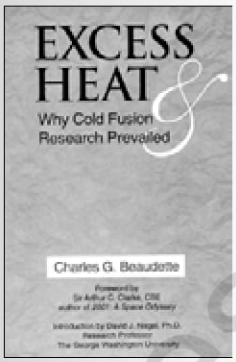

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The Philosophical Background to Einstein's Search for a Unified Field Theory

Ramon Prasad*

Is there a profound connection between the electromagnetic and the gravitational fields? There is some interaction because the deflection of a light beam at grazing incidence to the Sun and the shifting of spectral lines in light emerging from the Sun is a gravitational cause affecting the electromagnetic field. Could a reciprocal interaction enable the electromagnetic field to influence directly the gravitational field? Is the connection such that we should say that instead of having an electromagnetic theory and a gravitational theory, we have a unified field theory? Four eminent mathematical physicists in the first half of the twentieth century all believed that this question should be given an affirmative answer. The efforts of Weyl, Eddington, Einstein, and Schrödinger, the founding fathers of unified field theory, were closely related and are here given a brief survey.

Even at the time of writing down the gravitational field equations, with the right-hand side essentially equal to the energy-momentum tensor, Einstein said:

> *The right-hand side is a formal condensation of all things whose comprehension in the sense of a field theory is still problematic. Not for a moment did I doubt that this formulation was merely a makeshift in order to give the general principle of relativity closed expression. For it was essentially not anything more than a theory of the gravitational field, which was somewhat artificially isolated from a total field of as yet unknown structure.*[1]

Einstein may have started thinking about unified field theory in 1916[2] while working on the article which gave the first full exposition of the general theory. Two years later in 1918 he wrote in a letter to Weyl, commenting on the latter's paper "Gravitation and Electricity":[3]

> *And now you have actually given birth to the offspring that I was absolutely unable to produce—the construction of Maxwell's equations from the metrical tensor.*[4]

This was not quite so, as Weyl made clear in his re-formulation of the theory in his book *Space-Time-Matter*:

> *The metrical character of a manifold is characterized by two fundamental forms; the quadratic differential form depending on the metrical tensor and a linear differential form depending on the electromagnetic potentials.*[5]

What Weyl was really doing here was to relate the electromagnetic field to the projective invariance of the theory (relativity of length) in a manner similar to the way in which Einstein had related the gravitational field to the invariance of the theory against changes in the co-ordinate system (relativity of position and direction). Einstein was enthusiastic about Weyl's book:[6] "It is like a masterful symphony. The concept of the work is magnificent."

A pattern now emerged that was to repeat itself many times for nearly forty years. The new theory would be discovered to have defects. Attempts to rectify the defects would lead to a new theory about which there would be initial enthusiasm. Then once again defects would appear and the new theory would be relegated to the starting point for another investigation. This pattern was unsatisfactory to many scientists of the day. They were anxious to use the new gravitational and quantum theories to make definite predictions which could be verified experimentally. Pauli wanted to know what had become of the perihelion of Mercury and the bending of light in Einstein's 1929 theory. In a review written in 1932 Pauli says:

> *Einstein's never-failing inventiveness as well as his tenacious energy in the pursuit of unification guarantees us in recent years, on the average, one theory per annum. It is psychologically interesting that for some time the current theory is usually considered by its author to be the "definitive solution."*[7]

The majority of the scientific community went with Pauli, but for the committed participants small reverses were of no consequence. They believed that the two fields were unified in nature and that finding the correct theory was a problem commensurate with that of founding the general theory or quantum mechanics.

Returning to a consideration of Weyl's theory, Einstein was concerned that the line element was no longer an invariant. This implied that the scale carried by physical bodies depended on their history. Shifts in spectral lines of atoms with a different history were to be expected, a result not in accordance with observation. Eddington[8] proposed a modification of Weyl's theory which attempted to deal with the problem of non-invariant lengths. The theory identified the electromagnetic field and the gravitational field as the anti-symmetric and symmetric components of the Ricci tensor, with a constant of proportionality in the latter case, which supplied the missing gauge standard. Weyl was not happy with this modification and Pauli wrote to Eddington:

> *In contrast to you and Einstein, I consider the invention of the mathematicians that one can found a geometry on an affine connection without a primary line element is of no significance for physics.*[9]

Nevertheless, Einstein wrote in letters to Weyl:

> *Eddington has created a beautiful framework without content. . .Eddington's idea must be carried through to the end. . .Above stands the marble smile of implacable Nature which has endowed us more with longing than with intellectual capacity.*[10]

Einstein carried through his own investigations using an affine connection very close to the one which Weyl had used in his book. The conclusion, however, was that:

> *The theory brings us no enlightenment on the structure of electrons: there were no singularity-free solutions, and it is impossible to derive the source-free Maxwell equations. Unfortunately for me the result of these considerations is that the Weyl-Eddington theories are unable to bring progress in physical knowledge.*[11]

He then turned to the proposal of Kaluza that the four-

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dimensional manifold be extended to a fifth dimension. Einstein wrote to him:

> *The idea of achieving a unified theory by means of a five-dimensional cylinder world never dawned on me. At first glance I like your idea enormously. The formal unity of your work is startling.*[12]

Although returning several times to the Kaluza theory, the theory ultimately was unable to produce anything interesting on elementary particles. In 1925 Einstein developed his own unified field theory by means of analogy with his gravitational theory. The variational principle which leads directly to the field equations of general relativity was generalized by making the variables non-symmetric instead of symmetric as in the gravitational case. Although the formal results were as desired, Einstein wrote in letters to Ehrenfest:

> *I have once again a theory of gravitation-electricity; very beautiful but dubious. This summer I wrote a very beguiling paper about gravitation-electricity but I now doubt again very much whether it is true. As a result of numerous failures, I have now arrived at the conviction that this road does not bring us closer to the truth.*[13]

He did not, however, give up, and for the next thirty years continued working on different versions. Schrödinger also adopted this version of unified field theory[14] and for many years they developed together what came to be known as the Einstein-Schrödinger unified field theories.

Einstein, at the same time as he was actively participating in unified field theory development, was also taking a great interest in the rapidly moving field of quantum theory. He saw no distinction between the two, and regarded the former as being a possible source for solutions to the "paradoxes" of the latter. He thought that over-determination of the field equations of gravitation and electromagnetism would permit only discrete solutions which could then be identified with the quantum conditions characterizing elementary particles. This would have embedded the discreteness of quantum theory in the continuum field theory. This vision was the reason why he did not accept the quantum theorists' view that probability was the ultimate concept of reality. In letters to Born he wrote:

> *I continue to believe that one must look for such an over-determination through differential equations and that the solutions no longer have continuum character. But how? I just can't manage to give solid shape to my favorite idea of comprehending the quantum structure from over-determination through differential equations.*[15]

In 1923 he published a paper with the title: "Does the Field Theory Offer a Possibility of Solving the Quantum Problem." He posed the question:

> *Can quantum conditions of natural processes be adequately described by a theory based on partial differential equations? Quite certainly; all we have to do is over-determine the field variables by equations.*[15]

Once again things did not turn out as he wished. He noted that the deduction was not as cogent as one might wish and that the equations set out did not really have any physical meaning. Nevertheless he re-affirmed his faith in the program and urged everyone to participate:

> *The paper will have achieved its purpose if it induces mathematicians to co-operate and if it persuades them that the road here embarked on can be pursued and should certainly be thought through to the end.*[15]

A number of mathematicians did take up this offer and have sorted out some of the unsolved problems in the mathematics of unified field theory. Among these we mention Eisenhart, who showed that two of the three terms in the Weyl supplement to the affine connection were unambiguously tied to the projective invariance of non-Riemannian geometry.[16] The relativity of length is therefore unavoidable once we move outside the domain of Riemannian geometry and the general theory. If the length scale is to be determined, then it requires some other method. Another mathematician, Hlavaty, solved the problem in the Einstein-Schrödinger theory of writing down in a surveyable tensorial form the affine connection.[17] Unfortunately it confirms the opinion of Pauli, given above with respect to the Eddington connection, that its complexity precludes a straightforward application to any physical problem. Thirdly we mention that Einstein's idea, that the unified field could tie itself up in knots and over-determine the system, resulting in quantum conditions, has been verified.[18] A simple form for the affine connection gives sets of partial differential equations and, when the condition of consistency is imposed on these, yields discrete eigenvalues or quantum numbers characterizing the evolving fields.

Finally, to paraphrase Einstein's beliefs about the possibility of finding a unified theory, which put him in sharp contrast to many of his contemporaries:

> *The final aim of physics is an approach to the unique solution, that of a unified and most comprehensive theory which should also be of the greatest possible logical simplicity. Experience will guide us aright, there is a correct path and, moreover, it is in our power to find it. In nature is actualized the ideal of mathematical simplicity and pure mathematical construction enables us to discover the concepts and the laws connecting them, which give us the key to the understanding of the phenomena of nature. An agreement with experiment remains the sole criterion of the serviceability of a mathematical construction for physics. The truly creative principle resides in mathematics. In a certain sense, therefore, I hold it to be true that pure thought is competent to comprehend the real, as the ancients dreamed.*[19]

About the Author

Ramon Prasad did undergraduate and research degrees in theoretical physics at London University from 1958 to 1964. He worked in Paris for three years with J.P. Vigier at The Institute Henri Poincaré, on elementary particle physics. He returned to London in 1967 to take up a post teaching mathematics in the Faculty of Engineering, Science and Mathematics at Middlesex University. He has published four research monographs on Unified Field Theory and is currently working on a fifth.

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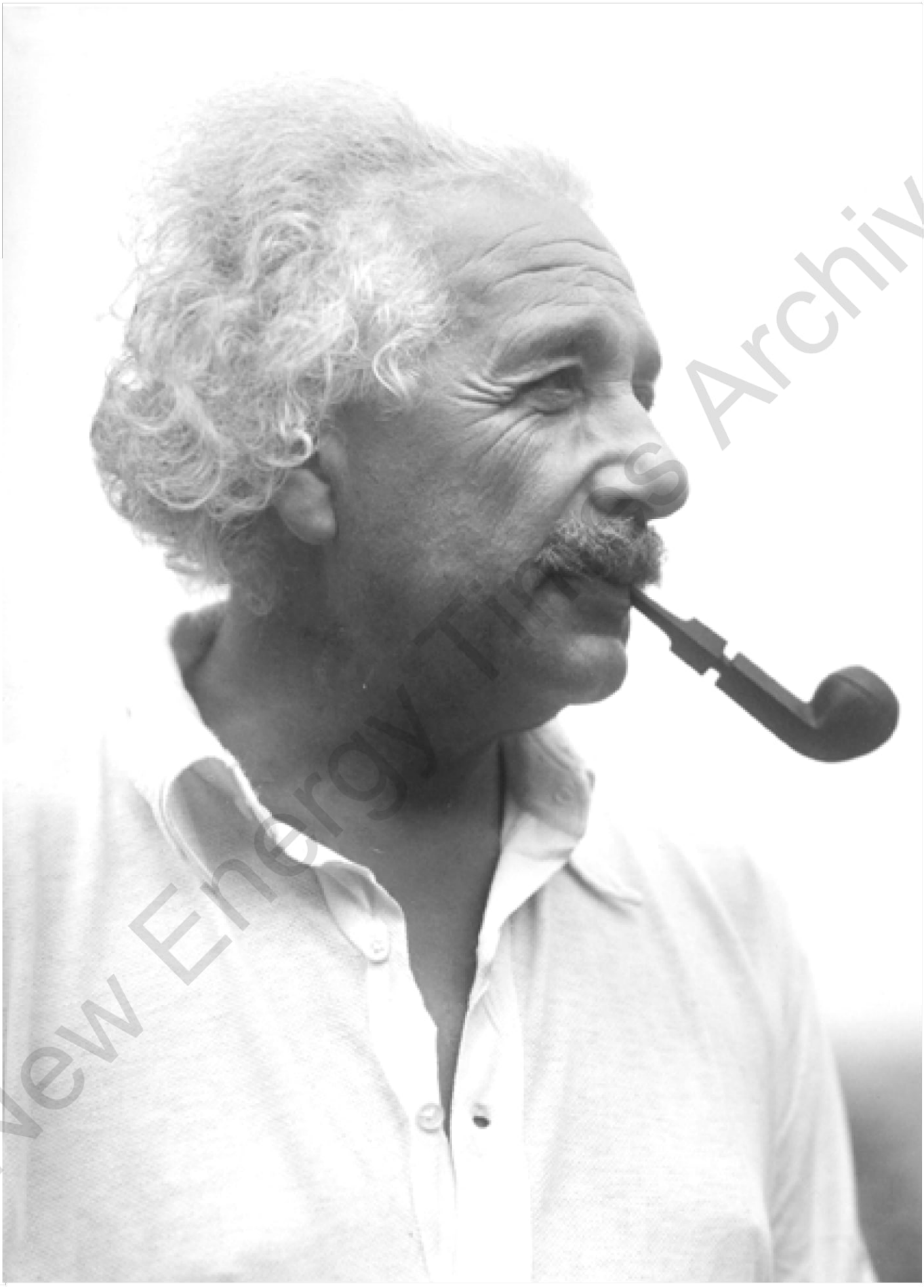

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The Eternally Valid Concepts in Einstein's Work

Domina Eberle Spencer*

Many reputable scientists have been critical of the validity of both Einstein's special and general theories of relativity, especially in the last half of the twentieth century. However, Einstein had the foresight to base these theories on *clearly defined postulates*. It is to his eternal credit that he recognized the importance of basing his development of the theory of relativity on definite postulates.

1. The Postulate on the Velocity of Light

Einstein[1] himself modified his original postulate that the velocity of light is always constant only two years after it was first proposed in 1905:

Postulate I Einstein (1907)*

The velocity of light in free space is a constant c, irrespective of the motion of source or receiver, in any inertial coordinate system.

According to Postulate I*, Figure 1, the radius r_I of the wavefront is assumed to be totally independent of the motion of the source of light, even though the source moves along an arbitrary curved path. The radius r_I is defined as the distance from where the source was at the time of emission τ, with rectangular coordinates $\xi(\tau)$, $\eta(\tau)$, $\zeta(\tau)$ to the point x(t), y(t), z(t) where the receiver is at time t. Thus,

$$r_I = \sqrt{\left(x(t) - \xi(\tau)\right)^2 + \left(y(t) - \eta(\tau)\right)^2 + \left(z(t) - \zeta(\tau)\right)^2} \,. \qquad (1)$$

The next year Walther Ritz[2] suggested an alternative ballistic postulate:

Postulate II Ritz (1908)

The velocity of light is always a constant c relative to the source at the instant of emission.

According to Postulate II*, Figure 2, as viewed in an inertial coordinate system, the center of the wavefront moves along a line tangent to the path of the light source at the instant of emission τ, at a velocity c plus the velocity $\mathbf{v}(\tau)$. Thus the radius of the wavefront, according to Postulate II is

$$r_{II} = \sqrt{\begin{array}{l}\left(x(t) - \left(\xi(\tau) + v_x(\tau)(t-\tau)\right)\right)^2 \\ +\left(y(t) - \left(\eta(\tau) + v_y(\tau)(t-\tau)\right)\right)^2 \\ +\left(z(t) - \left(\zeta(\tau) + v_z(\tau)(t-\tau)\right)\right)^2\end{array}} \,. \qquad (2)$$

Moon and Spencer[3] proposed the Universal Time Postulate in 1956. It was slightly revised by Moon, Spencer, and Moon[4,8] in 1989 and 1990.

Postulate III Moon and Spencer (1956) and Moon, Spencer, and Moon (1989) and (1990)*

In an inertial coordinate system in which the source is not in motion, the velocity of light is a constant c.

According to Postulate III*, Figure 3, in an inertial coordinate system in which the source is in motion, the center of the wavefront always remains at the light source. Therefore the radius of the wavefront is

$$r_{III} = \sqrt{\left(x(t) - \xi(t)\right)^2 + \left(y(t) - \eta(t)\right)^2 + \left(z(t) - \zeta(t)\right)^2} \,. \qquad (3)$$

Note that in the special case of linear motion, Postulate III* reduces to Postulate II. For the first time, Postulate III* thus provides a way of *unifying the ballistic theory of light preferred by Newton and the wave theory preferred by Huygens*.

Many papers were devoted to the analysis of experiments using these three postulates: astronomical observation of the binary stars,[5,6] the Michelson-Morley experiment,[7] the Michelson-Gale experiment,[8] and the Sagnac experiment.[9] Ritz's postulate must be abandoned because of the binary star observations unless space is assumed to be Riemannian rather than Euclidean. However, it was not until 1996 that an astronomical experiment was studied,[10] stellar aberration, which was inconsistent with Einstein's postulate on the velocity of light and was consistent with the Universal Time Postulate. It has been pointed out by Tom Van Flandern[11] that the very existence of the Global Positioning System in which signals from many moving clocks are accurately synchronized is the strongest proof of the validity of the Universal Time Postulate.

2. The Force Between Moving Charges

The second postulate of the special theory of relativity is the assumption that the Maxwell equations are universally valid. Einstein appears to have never questioned the Maxwell equations though Maxwell[12] himself called the equation for the force between moving charges the *keystone of electrodynamics,* and devoted the last chapter of his two volume treatise to the study of the force equations known to him. Gauss[13] in 1845 was the first to suggest that *all of electrodynamics should be derived from a single equation for the force between moving charges*. Gauss suggested two important criteria in letters[13] to his friend and collaborator, Wilhelm Weber:

First Gaussian Criterion (1835)

The force between moving charges must be expressed as a function of the relative velocities of source and receiver.

Second Gaussian Criterion (1845)

The equation for the force between moving charges must take into account the fact that electromagnetic effects are propagated at a very large but finite velocity.

However, Gauss never published an equation for the force between moving charges. He did propose an equation which satisfied the four Ampere experiments and was a function of relative velocities of source and receiver in a letter[13] to Weber. As seemed natural at that time, Gauss assumed that the force between moving charges must be in the radial direction. His equation was a function of position and relative velocity. It satisfied the first Gaussian Criterion but not the second so he did not consider it to be publishable. Weber[14] added an acceleration term to the Gauss equation and published it under his own name even though it

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did not satisfy the second Gaussian criterion but *implied that the velocity of light is infinite.*

$$\mathbf{F}_W = \mathbf{a}_r \frac{Q}{4\pi\varepsilon_o}\left[\begin{array}{l}\dfrac{1}{r^2}\left[1+\dfrac{(\mathbf{v}-\mathbf{u})\bullet(\mathbf{v}-\mathbf{u})}{c^2}-\dfrac{3}{2c^2}\left((\mathbf{v}-\mathbf{u})\bullet\mathbf{a}_r\right)^2\right]\\ -\dfrac{1}{c^2 r}\left[\left(\dfrac{d\mathbf{v}}{dt}-\dfrac{d\mathbf{u}}{dt}\right)\bullet\mathbf{a}_r\right]\end{array}\right]. \tag{4}$$

In the twentieth century, in 1926, Vannevar Bush[15] independently discovered the Weber equation.

The equation for the force between moving charges which is consistent with the Maxwell equations is the classical equation,

$$\mathbf{F}_C = \frac{Q}{4\pi\varepsilon_o\left(1-\dfrac{\mathbf{v}(\tau_I)\bullet\mathbf{a}_I}{c}\right)}\left[\begin{array}{l}\dfrac{1}{(r_I)^2}\left[\mathbf{a}_I-\dfrac{\mathbf{v}(\tau_I)\left(\mathbf{v}(\tau_I)\bullet\mathbf{a}_I\right)}{c^2}+\dfrac{\mathbf{u}(t)\times\left(\mathbf{v}(\tau_I)\times\mathbf{a}_I\right)}{c^2}\right]\\ +\dfrac{1}{c^2 r_I}\left[-\dfrac{d\mathbf{v}(\tau_I)}{d\tau_I}+\dfrac{\mathbf{u}(t)}{c}\times\left(\dfrac{d\mathbf{v}(\tau_I)}{d\tau_I}\times\mathbf{a}_I\right)\right]\end{array}\right] \tag{5}$$

This equation satisfies the second Gaussian equation, using Postulate I*, but it fails to satisfy the first Gaussian criterion: it is *not defined in terms of relative velocity but absolute velocity.*

Moon and Spencer began their search for a more satisfactory equation for the force between moving charges in 1954.[16] More than a hundred possible equations were considered. The most promising is an equation which satisfies both Gaussian Criteria which we have called the new Gaussian equation.[17]

$$\mathbf{F}_G = \frac{Q}{4\pi\varepsilon_o}\left[\begin{array}{l}\dfrac{1}{(r_{III})^2}\left[\mathbf{a}_{III}-\dfrac{\left(\mathbf{v}(\tau_{III})-\mathbf{u}(t)\right)\times\left(\left(\mathbf{v}(\tau_{III})-\mathbf{u}(t)\right)\times\mathbf{a}_{III}\right)}{2c^2}\right]\\ -\dfrac{1}{c^2 r_{III}}\left[\dfrac{d\mathbf{v}(\tau_{III})}{d\tau_{III}}+\dfrac{\left(\mathbf{v}(\tau_{III})-\mathbf{u}(t)\right)}{2c}\times\left[\dfrac{d\mathbf{v}(\tau_{III})}{d\tau_{III}}\times\mathbf{a}_{III}\right]\right]\end{array}\right]. \tag{6}$$

This equation satisfies both Gaussian criteria and is based on the Universal Time Postulate, Postulate III.*

How do we decide which of these three equations for the force between moving charges is actually valid? Is it enough to base our acceptance on the Gaussian criteria? Or should we seek actual experiments on which to base our conclusions?

According to the classical equation, the force between current elements is always perpendicular to the current element on which it acts. However, in a series of experiments performed over a period of ten years at M.I.T., Peter Graneau[18] and his son Neal Graneau[19,20] have demonstrated the *existence of tangential forces.* They believe that these forces are represented by the Weber equation. Andre Assis[21] has written a beautiful book in which he shows how the Graneau experiments can be predicted from the Weber equation. Does this mean that the Weber equation has been proved to be correct? Not at all. Tangential forces are also predicted by the new Gaussian equation and may explain the Graneau experiments as well as the Weber equation. This question should be investigated in detail. What it does mean is that *there is something radically wrong with the classical equation, for it predicts that there are never any tangential forces on current carrying conductors.*

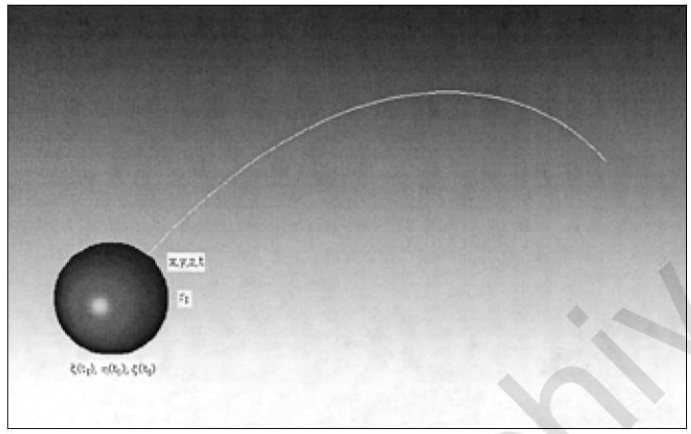

Figure 1. The wavefront according to Postulate I*.

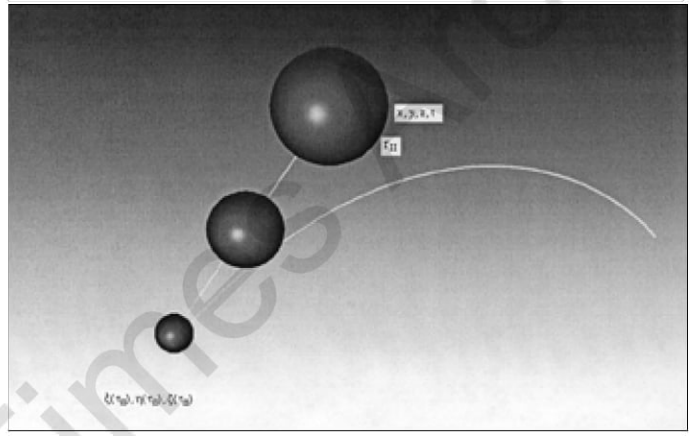

Figure 2. The wavefront according to Postulate II*.

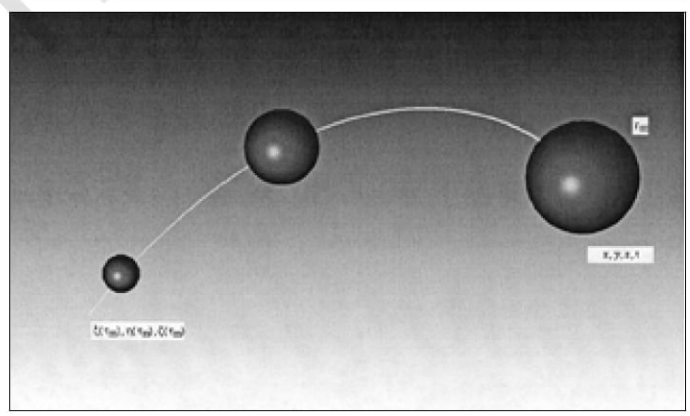

Figure 3. The wavefront according to Postulate III*.

Two Ph.D. theses at the University of Connecticut have attempted to find other experimental proofs of which force equation is valid. In 1992 Gerard Coutu[22] studied problems in magnetohydrodynamics. The analysis of the Hering Furnace[23] gave a qualitative verification of the experimental results for the new Gaussian equation. The Classical equation predicted that the Hering furnace, which has been used for nearly a century, could not operate. The Weber equation predicted that the force was in the opposite direction to that observed.

Overhead welding has been observed by every welder in the world. The question is what force[24] keeps the weld bead from falling. According to the classical equation for

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the force between moving charges, there is no force to hold the weld bead in place. According to the new Gaussian equation there is a force which holds the weld bead pressed tightly against the weld rod and the weld joint. According to the the Weber equation the force is again in the wrong direction.

In 1998, Philip Mann[25] studied the unipolar generator. The results were presented[26] in terms of three special cases: Case I-The copper disc rotates in a stationary magnetic field. Case II- The copper disc is stationary but the magnetic field rotates. Case III-Both the copper disc and the magnetic field rotate at the same angular velocity.

The Weber equation fails in all three cases. The Classical equation fails in two out of three cases. Only the new Gaussian equation gives the correct qualitative results in all three cases.

Thus we can conclude that of the three equations for the force between moving charges *only the new Gaussian equation satisfies both Gaussian Criteria and gives qualitatively correct results for the Hering furnace, overhead welding, and the unipolar generator.* It would be an even stronger case if the experiments analyzed were quantitative rather than qualitative. Yet we have very strong evidence that there is a major flaw in both the Weber equation and the Classical equation for the force between moving charges. This means that *Einstein's assumption of the perfection of the Maxwell equations is not justified.*

3. The Basic Postulate of General Relativity

When Einstein extended his theory of relativity to gravitation, he postulated that the motion of planets and of light in the vicinity of masses is along a geodesic in a curved Riemannian space.

The *two experimental proofs of the general theory of relativity are the advance of the perihelion of Mercury and the bending of light rays in the gravitational field of the Sun.* Andre Assis[27] has beautifully explained the advance of the perihelion of mercury by assuming that the mathematical form of the Weber equation describes the gravitational force between two masses. He uses Weber's equation to replace Newton's equation which says that there is a force of attraction which is always in the radial direction and varies inversely as the square of the separation of the masses, by Weber's equation which says that there is an additional force which depends on the relative velocity of the masses and on the rate of change of the relative velocity. With this assumption, Assis is able to successfully predict the advance of the perihelion of Mercury. It would appear that, though the Weber equation fails when applied to some electromagnetic problems, *the same mathematical equation is successful when applied to gravitational problems.*

The wide acceptance of Einstein's theory of relativity is based primarily on the acceptance of Eddington's interpretation of the data on the 1919 eclipse which were interpreted as confirming Einstein's theory that light was bent in the vicinity of a large mass. A very important paper by Marmet and Couture[28] has recently analyzed the validity of these conclusions. In this paper, the many inaccuracies of the original eclipse data are carefully analyzed. In addition, the Viking relativity experiment is studied. Another measurement of gravitational deflection uses very-long-baseline interferometry. *None of these experiments substantiates the effects predicted by general relativity when light passes near the Sun.*

Thus, we can conclude that *there appears to be no sound basis for believing in the validity of the postulate of general relativity.*

4. The Eternally Valid Concepts of Einstein

The first and most important characteristic of both special and general relativity is that the entire subject is *based on three clearly stated postulates*. Einstein is to be greatly complimented for having effectively used the *postulational approach*. It is not surprising that each of his three postulates are now shown to require revision.

The second eternally valuable idea which Einstein introduced is the effectiveness of *representing physical quantities by tensors*. Without his use of tensors in relativity the entire subject might have been left an obscure branch of mathematics, useful primarily in differential geometry. The popularity of tensors is largely attributed to Einstein's use of them in relativity. The fact that tensors must be generalized to *holors*[29] in order to describe all physical quantities was not recognized during Einstein's lifetime.

The entire study of tensors and holors will forever be enriched and simplified by Einstein's simple idea of omitting summation signs whenever subscripts and superscripts are repeated, the *Einstein summation convention*.

Thus, we can be eternally thankful to Einstein for his insistence on the use of the *postulational approach* in physics, for his introduction of the *representation of physical quantities by tensors,* and for the introduction of the *Einstein summation convention.*

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About the Author

Dr. Spencer was born in New Castle, Pennsylvania on September 26, 1920. She graduated from Friends Select School (Philadelphia) in 1937. She further received a S.B. in physics from M.I.T. in 1939 with thesis direction under J.A. Stratton, a S.M. in mathematics from M.I.T. in 1940 with thesis direction under D.J. Struik, and a Ph.D. in mathematics with minor in electrical engineering in 1942 with thesis direction under D.J. Struik. Dr. Spencer was an Assistant Professor of Physics at American University, Washington, D.C., from 1942-1943, Tufts College,Medford, Massachusetts from 1942-1947, and Brown University, Providence, Rhode Island from 1947-1950. She was an Associate Professor of Mathematics at the University of Connecticut in Storrs, Connecticut from 1950 to 1960. Dr. Spencer has been Professor of Mathematics at the University of Connecticut since 1960. She is author of eight scientific books with Prof. Parry Moon of M.I.T. and nearly three hundred scientific papers.

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Seeing Einstein in a New Light

John Yemma

Reprinted with permission from the Boston Globe Magazine, August 9, 1998

Three days a week for the past forty-eight years—stretching back to a time when Studebakers and Packards plied the modest highway system of New England—Domina Eberle Spencer has climbed into her car and driven from Boston to Storrs, Connecticut, and back. Through the 1950s and 1960s, Sputnik and Vietnam, the 1970s and 1980s and deep in the 1990s—through rain, snow, glorious autumns, parched summers, better cars, busier roads—Spencer has commuted to her job as a professor of mathematics at the University of Connecticut, one of the few first-rate universities that would employ a woman with a doctorate in math in the rigidly sexist climate of the 1950s.

It is more than two hours in each direction, so Spencer has done a lot of thinking over the years. She is not a person who thinks idle thoughts, however. Her mind moves easily over differential equations, vector analysis, relativity, and the concept of time.

Now 77, Spencer has come to some interesting conclusions as a product both of drive time and of collaboration with academic colleagues, primarily her late husband, Parry Moon, who was a professor of electrical engineering at the Massachusetts Institute of Technology.

One of her conclusions is that Albert Einstein was wrong. She does not say this lightly. She is not a pseudoscience gadfly or a wishful thinker. She has labored over equations and analyzed proofs.

"I think Einstein was a great man to say you needed a postulate on the velocity of light," Spencer says. "But he had the wrong postulate."

A woman with deeply intelligent eyes and a courtly, almost Southern cadence in her speech, Spencer grew up in western Pennsylvania and as a child was home-schooled in math by her older sister. When she talks, she moves among the thoughts of the great mathematicians and physicists: Euclid, Isaac Newton, James Maxwell, Michael Faraday, K.F. Gauss, and Einstein. She terms her work—and theirs—"natural philosophy," not physics, which she considers too limiting.

Spencer says this as she sits on the terrace of a high-rise apartment located just at the base of Mission Hill. She is wearing a necklace of purple stones and cradling a tiny Chihuahua named after Hypatia of Alexandria, the first known female mathematician. Brilliant and outspoken, Hypatia was murdered by Alexandrian monks in AD 415.

Spencer has developed what she calls the "universal time postulate." She and her collaborators arrived at it by analyzing what is called the "twin paradox," in which a set of twins accelerating away from each other wants to synchronize their clocks. They would do so by signaling the time to each other using light, which is the fastest way of sending a signal. Imagine they are signaling with flashlights.

Light moves at 186,000 miles per second, and, according to Einstein, it is a constant. You can't add to its speed, even if you are moving when you send the light signal. And that's where the problem comes in. The twins' clocks can never be synchronized. Even at light speed, time will always have elapsed by the time the light signal is sent and received. And yet, Spencer says, clocks do get synchronized. The clocks on satellites, she says, can be precisely synchronized with other satellites and with terrestrial clocks.

Spencer's universal time postulate is that, in effect, light can go faster or slower than 186,000 miles per second, depending on the motion of the twins with their clocks. The "instantaneous velocity" of the twin who is sending the light signal at the instant of reception by the second twin gets added to the velocity of light.

> **"If people worship science and think it is fixed for all time, that it is right, and they know it, they are all making a great mistake."**

The essence of Spencer's message is that everything in our universe is in constant motion—planets, galaxies, clocks, long-distance commuters—and that Einstein was not taking this fully into account in his special theory of relativity.

Einstein's view of the physical universe reigns in modern science, but it does produce strange results, in which, for instance, moving clocks run faster than stationary ones, moving objects are shorter than objects at rest, and moving objects weigh more than objects at rest.

If Spencer is right about "universal time," Einstein is wrong. Needless to say, the scientific community has not jumped up and applauded—not yet, perhaps never. Though unable to judge her theoretical work, her thesis adviser when she was at MIT—103-year-old Dirk Jan Struik, a professor emeritus of mathematics—calls Spencer a "most accomplished scientist and engineer."

But is she out of her depth? Almost every physicist working today would say so. Officials of the American Association for the Advancement of Science have rejected numerous bids by Spencer's supporters to present their case at AAAS national meetings. Calling special relativity "bread and butter in particle physics," Nobel laureate physicist Leon Lederman, of the Fermi National Accelerator Laboratory, in Batavia, Illinois, says Einstein is in no danger of being dethroned.

Undaunted, Spencer is working on even more complex equations in the field of electrodynamics that challenge conventional thinking. "If," she says, "people worship science and think it is fixed for all time, that it is right, and they know it, they are all making a great mistake."

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This is what Spencer ponders each time she pilots her car on the Massachusetts Turnpike. She drives a car she has named Maxwell; she has a spare car named Gauss. Hypatia rides beside her. Whether she is ultimately judged right or wrong on her scientific postulates, no one can say that she lets her mind idle.

A Reader Replies to the Yemma Article

The following letter from John Chappell, Jr., in response to the Yemma article about Domina Eberle Spencer, appeared in the September 27, 1998 issue of *Boston Globe Magazine*.

I am less troubled by John Yemma's rather imperfect outline of Domina Eberle Spencer's trans-Einsteinian physics than by the fact that he cites only one outside expert, and that man, Leon Lederman of the Fermi Accelerator Lab, speaks against it.

He could have found someone much more sympathetic with her views among veteran workers at this same lab: Delbert Larson, who designed one of its accelerators with no recourse at all to the special theory of relativity (SR), thus refuting the common claim that SR must be correct because working accelerators depend on it.

Larson used instead a Lorentzian-type theory, which employs the same equations as does SR, but avoids irrationalities such as time dilation and twin paradoxes. He belongs to the Natural Philosophy Alliance, the group repeatedly denied time on the national programs of the American Association for the Advancement of Science, which Yemma describes too narrowly as "Spencer's supporters." It promotes the ideas of many members, among whom Spencer is one of the most accomplished.

Still another alternative way of explaining accelerator processes is a Gaussian-type additive—velocity theory, such as Spencer and others offer. The main key to deconstructing SR's ridiculous house of cards is to realize that most evidence, even in the hard sciences, can be interpreted in more than one way; and ways that deny the principles of logic, as SR does, are the least satisfactory.

John E. Chappell, Jr.
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IN THE NEXT ISSUE
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cy established in 1978: "Sophistication in mathematical reasoning and technology that has accompanied progress in particle physics no longer allows an ordinary mortal to pursue the science both in an experimental laboratory and in the quiet of a study, as in the good old days of Faraday, Cavendish, and Rayleigh, or even in the more recent times of Enrico Fermi." (*Theoretical Physics at Fermilab*, NALREP, March 1978.)

These folks believe the DOE's monumental experimental apparatuses such as NIF, Z-pinch, Fermilab, TFTR, ITER, NOVA, SSC, and NSTX are modern cathedrals and view themselves as Renaissance men. Robert Wilson, father of Fermilab, wrote that he ". . .found a striking similarity between the tight community of cathedral builders and the community of accelerator builders: Both of them were daring innovators, both were fiercely competitive on national lines, yet both were basically internationalists." To this, Nobel Laureate Leon Lederman added: ". . . both are built at great expense as a matter of faith. Both provide spiritual uplift, transcendence, and prayerfully revelation. Of course not all cathedrals work."

DOE sophisticates allege second line universities, small laboratories, and geographical remoteness nurtures bad science, sick concepts, and crackpot scientists. We are cited for all these allegations by the USPTO. The facts are: Princeton's monster TFTR failed to ignite hot fusion, 100 million degrees Celsius. So, the DOE funds a new Princeton experiment, the NSTX to study ignition. It will fail. LLNL's NOVA failed to ignite hot fusion. NIF, a new LLNL multi-billion dollar experimental cathedral, is being built to study ignition. It will fail. Sandia's PBFA-II and successive experiments have failed to ignite hot fusion. Sandia seeks $400 million to build a new experimental "Z-pinch" experiment. The DOE has spent $25 billion dollars and forty-five years experimenting with hot fusion and yet fusion ignition is not in sight.

But on August 1, 1995, Dr. Dennis Cravens ignited cold fusion in the Ragland triode cell and sustained the fusion reaction for two hours. He reignited fusion in the apparatus the next day. In my laboratory on March 21, 1996 cold fusion ignited in the triode apparatus. The fusion reaction sustained for five months until deliberately shut down on August 23, 1996. Details of these successful experiments were reported in my ICCF-6 paper on October 13, 1996. On February 24, 1998 Randall J. Hekman testified by affidavit to witness of excess power from triode test cells lasting up to two months. The USPTO denies these facts because ordinary mortals cannot be expected to cope with sophisticated fusion experiments.

It seems unreasonable to assert that the USPTO denies documented facts and testimony of ordinary citizens. But U.S. policy as seen by DOE is, "[DOE, then AEC] was given the mission, as a national trust, of funding and overseeing basic research in nuclear and what was later to become particle physics." The USPTO fiercely defends nuclear science as the exclusive province of the National Laboratories. Nowhere has this defense been more fierce, longer lasting, or irrational than in the denial of cold nuclear fusion.

The USPTO cites DOE documents, ERAB, Miller memo, etc., as national policy discrediting cold fusion.

Forty-five years of failed hot fusion research betray the national trust.

The DOE believes nuclear fusion is beyond ordinary intellect, the national laboratories are preeminent, that geographical remote second line research is wrong science, and for these reasons DOE research is immune to Congressional review. Without feedback or audit, all systems ultimately drift to a limit. I believe U.S. nuclear research drifts ever closer to catastrophe. We already have failed, to the Nation's detriment, to develop alternative energy resources and the failure of NIF, which seems likely, will significantly weaken national security.

The staff at *Infinite Energy* knows the arguments we make in favor of my patent claims. This letter is to make a record of larger proportions regarding nuclear research. It is not intended to alarm or hang crepe, but as a wake-up call to the fact that U.S. nuclear research is stalled and has been for over twenty years. I do not know how this letter may accomplish any worthwhile purpose, but I wanted to put my thoughts on paper for those to read who are knowledgeable of where I am coming from.

Evan Ragland
Diamondhead, Mississippi

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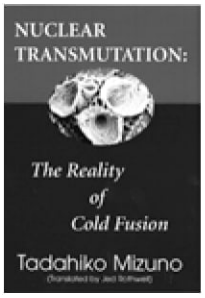

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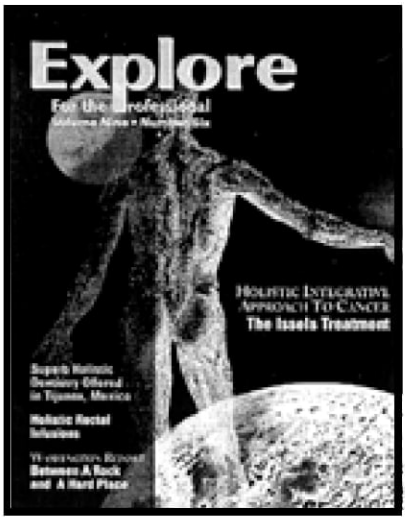

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